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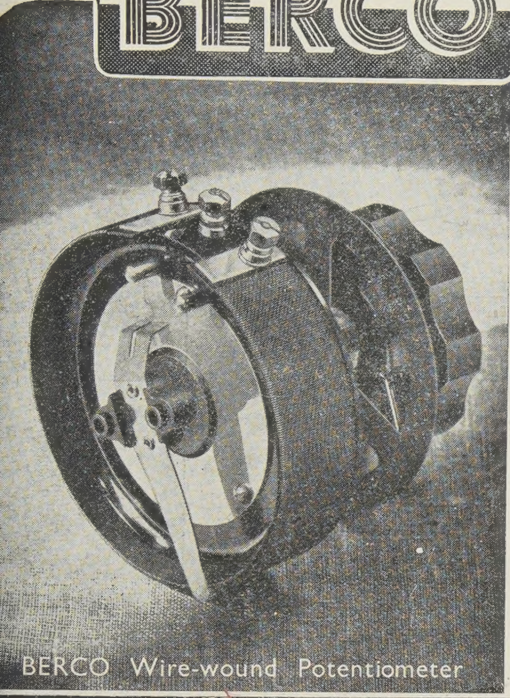

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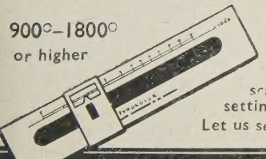

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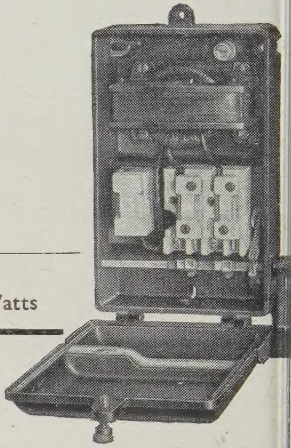

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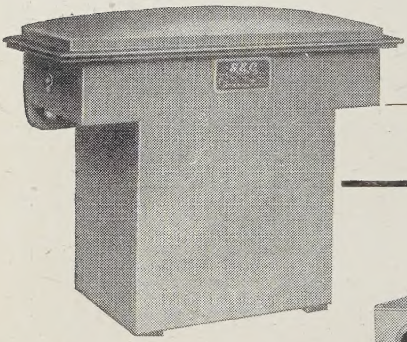

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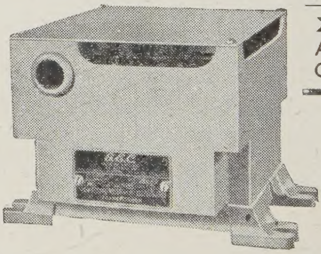

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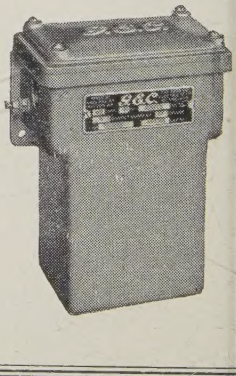

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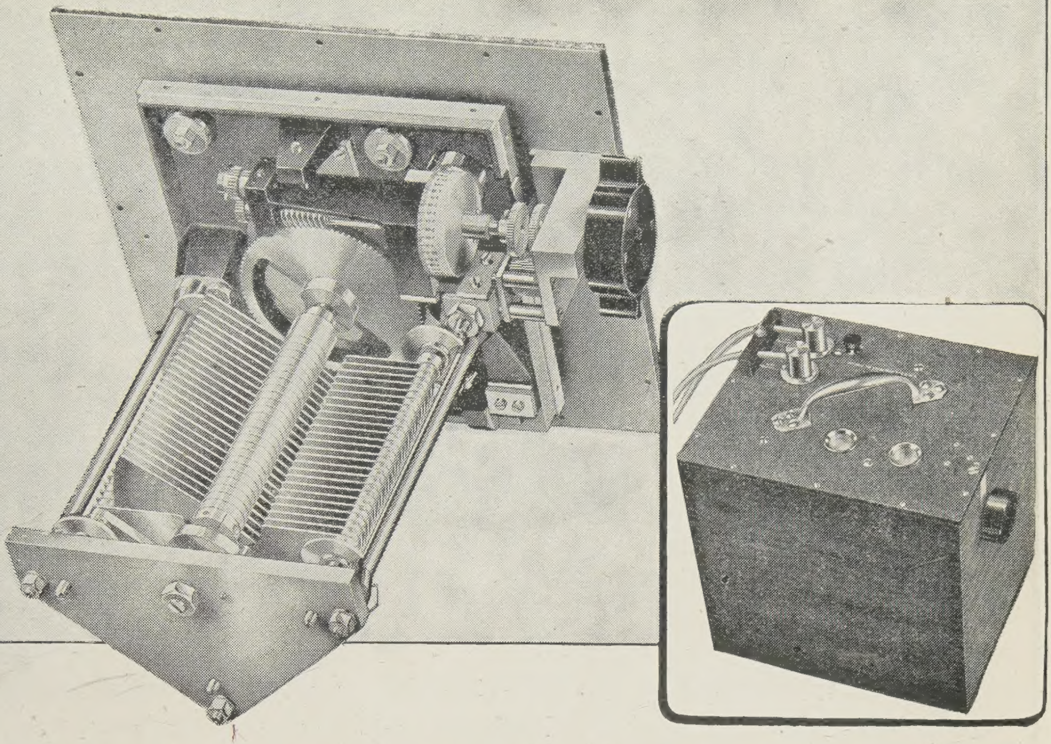

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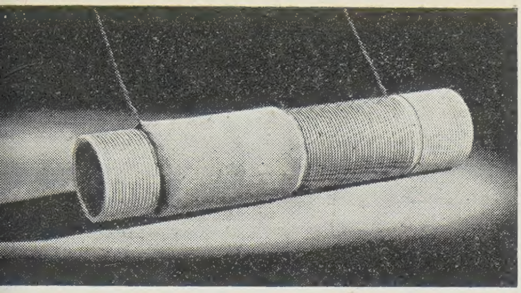

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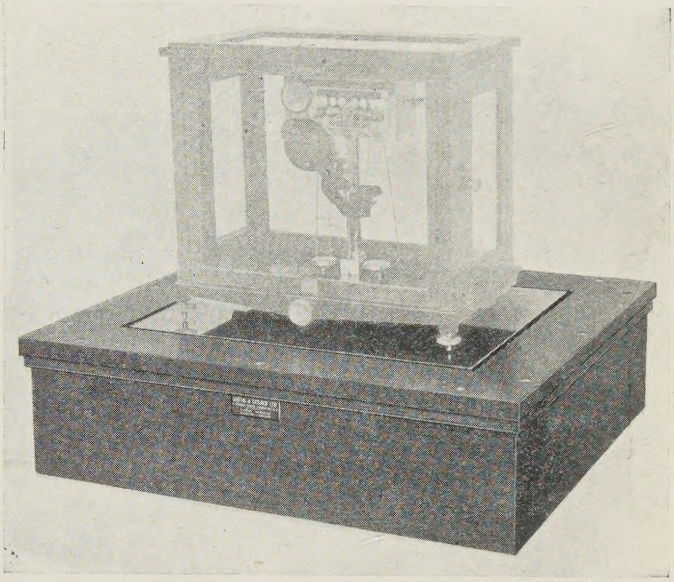

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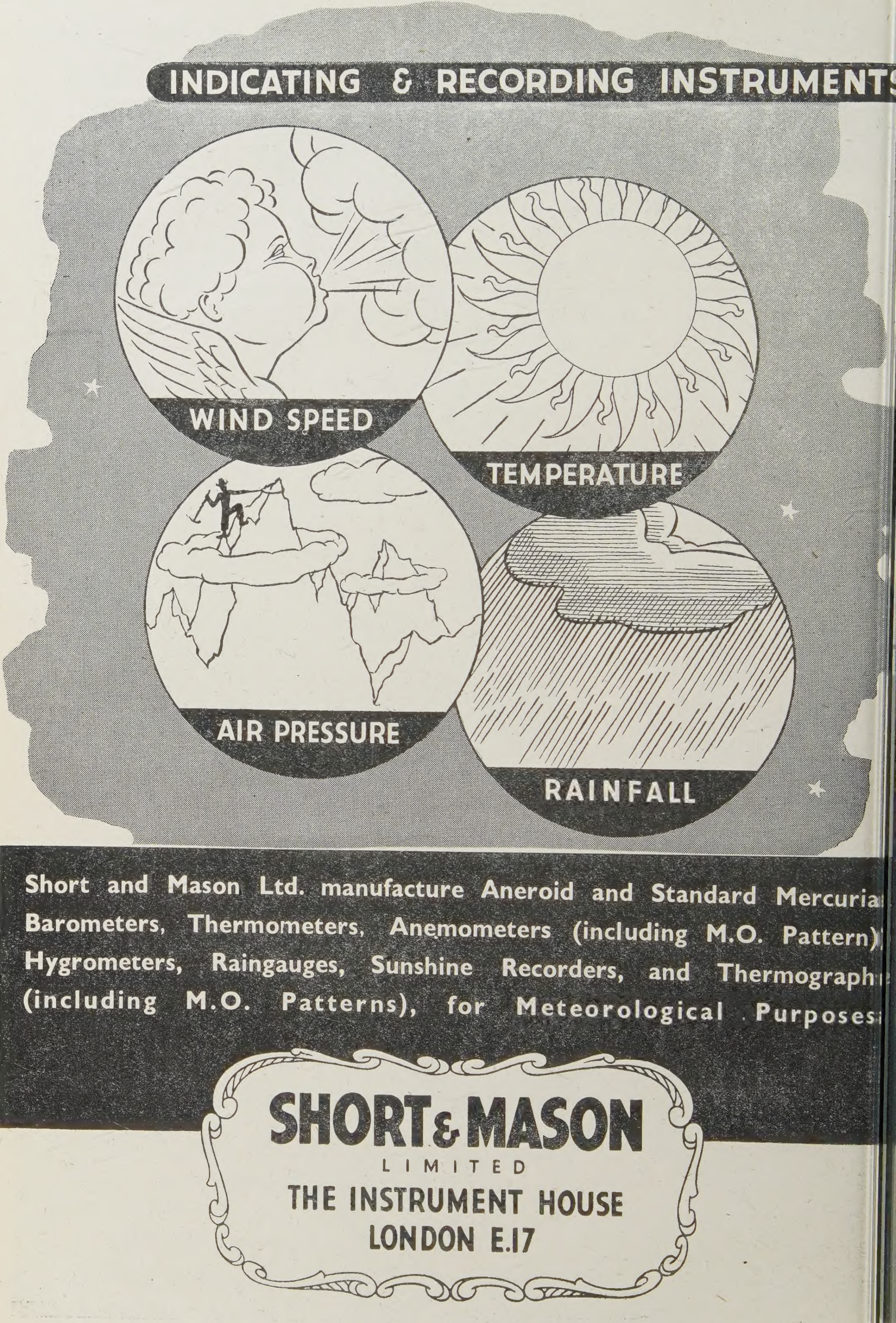

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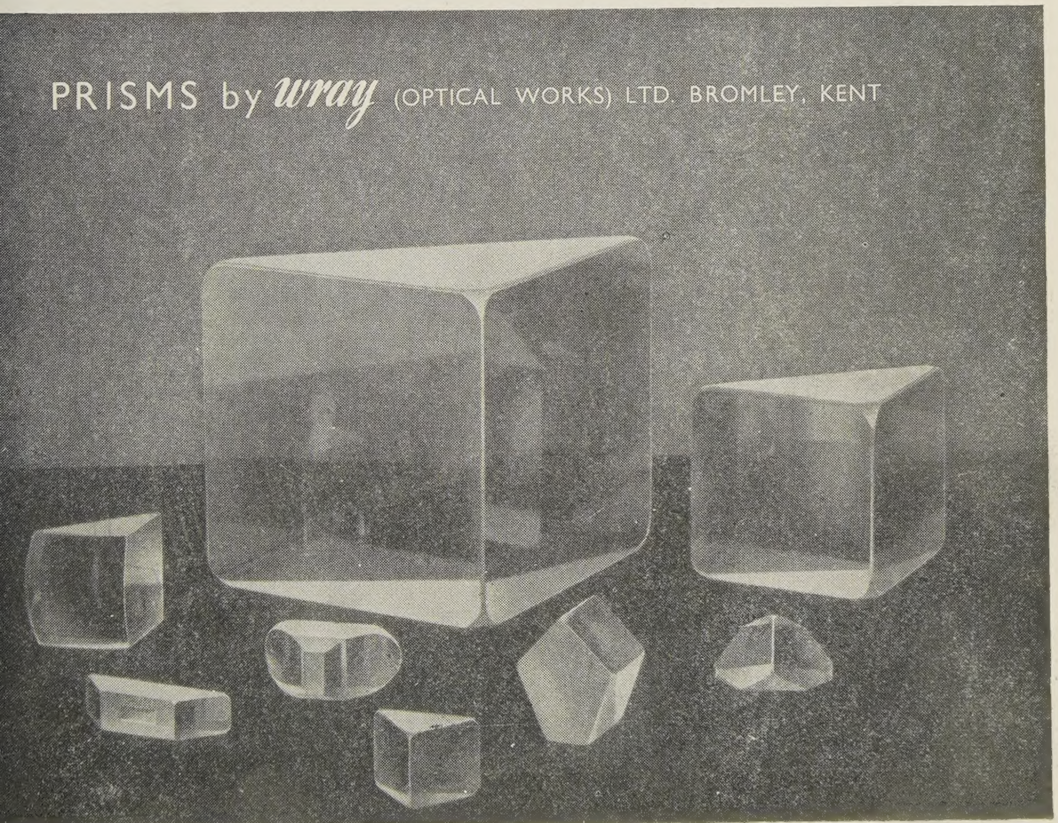

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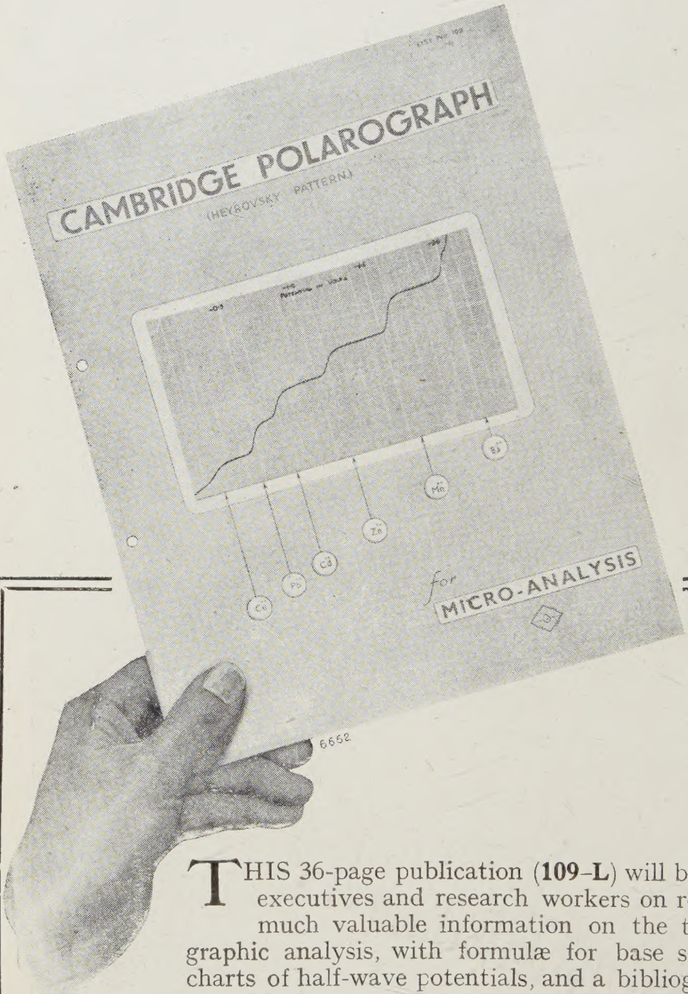

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THE PROCEEDINGS OF
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ON ASPHERIC ANASTIGMATIC SYSTEMS

By C. R. BURCH, B.A., PH.D.,
Bristol University

Paper read to the Optical Group 18 *September* 1942; *MS. received* 15 *March* 1943

ABSTRACT. The Seidel aberrations of aspheric systems are discussed by means of the " optical see-saw diagram " or diagram of " Schmidt plates ". The conditions for anastigmatism of a 2-mirror pair and a refracting singlet are deduced, and two flat-fielded anastigmatic singlets are designed.

§ 1. INTRODUCTION

THE purpose of this paper is to discuss the design of certain anastigmatic aspheric systems by the method of the "optical see-saw diagram". In this method, some applications of which I have described in *Mon. Not. R. Astr. Soc.* 1942, the algebraic treatment of the so-called third-order (Seidel) aberrations of axially symmetric aspheric systems is cast into a form peculiarly easily visualized in terms of simple and, indeed, practically important concepts, viz. Schmidt plates. When one conducts the extended-paraxial analysis in terms of these *natural co-ordinates*—as I regard them—the equations automatically assume a particularly suggestive form, and relations appear which, though they must also be contained in the equally exact analysis, e.g. of Conrady (1919), have remained to me, at least, hitherto invisible—shrouded in a dank miasma of algebraic artificial fog. I was led to the method by trying to describe concisely what I had done to a certain lens by adding to it a *Schmidt plate* (Burch, 1938), and what could be done by adding other hypothetical plates. I have since found that the basic underlying idea is by no means new, and that the *see-saw diagram* for a system of spherical surfaces was given by H. L. Aldis no less than 43 years ago. " Science moves but slowly, slowly creeping on from point to point".

I shall now describe the method in its generalized form *de novo*.

§ 2. THE CONCEPT OF ANASTIGMATIZING PLATES ("SCHMIDT PLATES")

If the spherical aberration of a refracting or reflecting spherical surface be corrected, not by figuring the surface itself but, as in the Schmidt camera, by placing at the centre of curvature thereof a figured thin plate or lamina having its principal retardation term proportional to the fourth power of zonal radius, the primary off-axis aberrations are, *ipso facto*, simultaneously annulled. The system is rendered anastigmatic in the extended paraxial region, and distortionless as between object and image surfaces concentric with the refracting surface.

The extended paraxial region we define as that region within which wave-front asphericities involving angular powers (whether of aperture or field) higher than the fourth may be neglected: a system is said to be anastigmatic when it produces spherical image wave-fronts provided powers of angular field higher than the second be neglected.

To see that anastigmatism is produced in the extended paraxial region between concentric object and image surfaces, it is sufficient to observe that the system is stigmatic on axis by definition; now the axis (of revolution symmetry) is the perpendicular through the centre of the Schmidt plate, so that the only errors with which an off-axis object-point is imaged are those due to the fact that the figured "plate" has *not* been re-oriented to make its central normal pass through the object-point. The resulting changes in the incidence heights of the various rays on the "plate" are at least of the second angular order, and so can cause no change of an order less than the fifth in the retardations they undergo, and similarly the changes in incidence angle on the "plate" can also affect no order below the fifth—and, therefore, cause negligible change in retardation of the emergent wave-front.

Next, we observe that anastigmatism is still produced in the extended paraxial region even when the object surface, though of revolution about the optic axis, is no longer concentric with the refracting surface. For the effect of a changed object-surface curvature is to produce a change proportional to field angle squared, in the distance of the object-point from the refracting centre; now the strength of the "plate" necessary for stigmatism is a continuous function of this distance, so that the departure from off-axis stigmatism due to a changed object-surface curvature is expressible as a change of the second order in the coefficient of a term already of the fourth order of smallness: accordingly it is negligible in the extended-paraxial region.

Notice also that the effect of "bending" a figured plate—i.e. of replacing a figured thin plane-parallel plate by a figured thin curved parallel plate—may also be neglected in the extended-paraxial region, for it produces at most a sixth-order change in the depth of figuring at the incidence point of a given ray and (because of the change of incidence angle) a fifth-order change of retardation of the emergent wave-front—and this holds whether or no the "plate" be placed at the centre of curvature of any refracting surface. So we may treat a figuring on a reflecting or refracting surface as another "plate" and ignore its curvature in the extended-paraxial region.

§ 3. THE IMAGING OF FIGURED PLATES

From what has been said it will be clear that in the extended-paraxial region we may replace any "plate" by its Gaussian image taken through any number of spherical surfaces and any number of other plates, for the approximation inherent in the Gaussian imaging process can, at most, cause a fifth-order error in the estimation of the retardation incurred by a given point on the wave-front. Accordingly we may confer a special simplicity on the fourth order (Seidel) retardations of any complete system by regarding each surface as anastigmatized by its own special "Schmidt plate" (cancelled if, in fact, no anastigmatizing plate is provided, by a plate of equal and opposite strength), and then imaging into any convenient representative space all the figured plates which the system possesses except those anastigmatizing individual surfaces. We have thus replaced the actual system by an anastigmatic system plus, in one conveniently chosen image space, a collection of figured plates, some of these being the "negatives" of missing anastigmatizing plates, and others figuring on surfaces which we have chosen to aspherize.

§4. THE SEE-SAW DIAGRAM

It conduces to further simplicity if we select for our *plate diagram* an image space in which the object is at infinity—"among the stars". I shall use the term *star-space* to describe such an image space. It is always permissible to add a hypothetical anastigmat of arbitrary focal length, to produce an artificial star-space, if no convenient star-space exists in the system as given. It will be apparent on further consideration that since we are concerned only with establishing a correspondence of collineations for the more convenient summation of retardations produced by the various plates, it is not essential that all the plates should be on the same side of this anastigmat. It is therefore permissible to let it act on an intermediate image, and we shall find this conducive to analytical brevity in an example given later.

If, now, these plate images be regarded as masses proportional to their strength, and positive or negative, as the case may be, poised on a see-saw in star-space, the pivot-point being the entrance pupil, then

(1) the spherical aberration is given by the total weight on the pivot-point;

(2) the coma is proportional to the unbalanced moment about the pivot-point;

(3) the astigmatism (and its associated change of focus) is proportional to the moment of inertia about the pivot-point;

(4) that part of the distortion not associable with the representation of spheres on planes is proportional to the third moment of mass about the pivot-point.

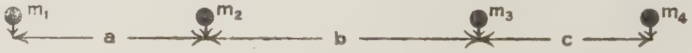
For consider a plate distant D from the pivot-point, and giving retardation Kr^4, r being zonal radius. Take Cartesian co-ordinates x, y, centred on the incidence-point on this plate of a principal ray making angle θ with the optic axis, and normal to the y-axis. The plate retardation $Kr^4 = K[(x+\theta D)^2+y^2]^2$ then contributes the following terms to the wave-front:

$$\left.\begin{array}{ll}
K\,(x^2+y^2)^2 & \text{spherical aberration,}\\
KD\;4x(x^2+y^2)\theta & \text{coma,}\\
KD^2\;2(x^2-y^2)\theta^2 & \text{astigmatism,}\\
KD^2\;4(x^2+y^2)\theta^2 & \text{associated change of focus,}\\
KD^3\;4x\,\theta^3 & \text{contribution to distortion,}\\
KD^4\;\theta^4 & \text{retardation of the whole wave-front.}
\end{array}\right\}\quad \ldots\ldots(1)$$

Thus the correction of the first three Seidel aberrations of an axially symmetric aspheric system resolves itself into the problem of annulling the first three moments of the see-saw, ΣK_r, $\Sigma K_r D_r$ and $\Sigma K_r D_r^2$, exactly as was done by H. L. Aldis for a system of spherical surfaces, and to correct distortion the procedure is again the same in both cases—one must give to $\Sigma K_r D_r^3$ the value necessary to equilibrate the distortion inherent in the change from a spherical to a flat object and image screen. But, although the procedure in the two cases is so similar, the kinds of question one wishes to answer with the see-saw diagram will in general be different in the spheric and aspheric cases. Let us therefore approach the aspheric see-saw diagram *de novo*, uninfluenced by the relative merits that experience may have taught us to attach to the see-saw and to other methods of describing the Seidel aberrations of spheric systems. The point is this: in the spheric case, the K's and D's of the diagram are not the independent primary physical variables in which we are interested, for their derivation by imaging into star-space will in general cause variation of each K and D to affect all the K's and D's associated with subsequent surfaces. In the aspheric case, on the other hand, some of the K's—all those, in fact, which represent strengths of actual figured surfaces—are independent primary variables which may be chosen at pleasure. So we are led at once to consider an existence theorem, for we ask: Can any choice of these "free" K's annul the first three moments of the diagram, or are further conditions of compatibility necessary in simple kinds of system? The theorem to which this led had, I found, already been given by Aldis as a proposition relating to spheric systems. Applied to aspheric systems it leads rapidly to an elegant solution of the problem of 2-surface anastigmat design.

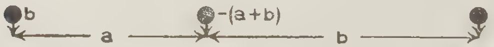
§ 5. THE ALDIS 4-PLATE THEOREM

The simplest anastigmatic system is the Amici lens, the see-saw diagram of which—in the Bellman's classic phrase—is "a perfect and absolute blank". Next comes the Schmidt camera, and the monocentric reflecting anastigmat, in which there are two equal and opposite coincident plates, and refracting analogues. Next come what might be termed monocentric Schmidt-Cassegrains, with three coincident plates of zero total strength. Only when we come to systems with at least four plates is it possible to achieve anastigmatism with non-coincident plates. We therefore consider the conditions for anastigmatism of such a system. That is, we enquire under what conditions the see-saw



has zero, zeroth, first and second moments.

Observe that



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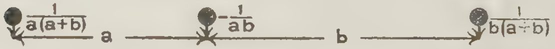
so that 

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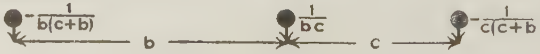

has second-moment minus unity.

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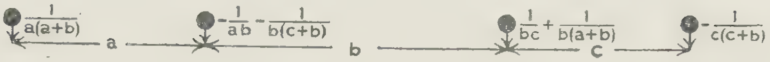
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$-\frac{1}{ab}-\frac{1}{b(c+b)}$
b
$\frac{1}{bc}+\frac{1}{b(a+b)}$
c
$-\frac{1}{c(c+b)}$

has second-moment zero,

i.e. $$bc(b+c);\quad -c(a+b)(a+b+c);\quad a(b+c)(a+b+c):\quad -ab(a+b) \qquad \ldots\ldots(2)$$

has second-moment zero, and the necessary and sufficient condition for anastigmatism of a 4-plate system is that m_1, m_2, m_3, m_4, if not zero, should have that ratio. Aldis, who obtained the theorem by a slightly different analytical approach, points out its geometrical interpretation, that (in terminology appropriate to our more general application) each plate must have strength inversely proportional to the product of its distances in star-space from all the others.

§ 6. THE TWO-SURFACE SYSTEM: CONDITION FOR ANASTIGMATISM

A system of two surfaces is in general a 4-plate system. Let us apply the foregoing result. We shall need an expression for the strength of the anastigmatizing plate required by a spherical surface. Consider a sphere of radius r, refractive index N', immersed in an ocean of index N. Let an image be produced, distant v inside the pole in N-space, by an object distant u inside the pole in N'-space.

Then, if terms involving sixth and higher powers of zonal height be dropped, it may be verified that the strength of the anastigmatizing plate required at the centre of curvature in N-space is given by

Retardation at zonal height h

$$\left.\begin{aligned} &= \frac{h^4}{8r^3}(N'-N)\left(\frac{N}{N'}\right)^2\left(\frac{u}{u-r}\right)^2\left\{\frac{Nu-(N+N')r}{Nu}\right\} \\ &= \frac{h^4}{8r^3}(N'-N)\left(\frac{N}{N'}\right)^2\left(\frac{v}{v-r}\right)^2\left\{\frac{Nv-(N+N')r}{Nv}\right\}, \end{aligned}\right\} \qquad \ldots\ldots(3)$$

while if the plate is placed in N'-space the factor $(N/N')^2$ must be replaced by

$(N'/N)^2$, and h by h', the zonal height on the plate in N'-space ......(4)

Let us take as the first surface of our system an "asphere" (i.e. a figured sphere) of paraxial radius ρ, refractive index N', immersed in an ocean of index N. Let the second surface, distant d inside the first, be an asphere of radius $-R$, index N', immersed in the ocean of N. Thus, if ρ, R, d are all positive, we have a meniscus lens of N' in N, while if $N'=-N$, we have a two-mirror system with a concave radius of ρ and a convex of R. It is necessary to specify the object-position in some manner, and we shall do so by means of the position of the intermediate image, since this image is referable with equal analytical simplicity to either surface. Let the intermediate image be distant L in N'-space inside the ρ-asphere. It will then be distant $-(L-d)$ "inside" the asphere of $-R$ (with the conventions here used, the paracentre of an asphere of paraxial radius k is distant k "inside" the asphere for all values of k). The strengths of the anastigmatizing plates required by the spheres ρ, $-R$ in N'-space are then given by setting $v=L$, $r=\rho$, or $v=-(L-d)$, $r=-R$, in (4). They are thus

Retardation of the "ρ-plate"

$$=\frac{(N'-N)h'^4}{8\rho^3}\left(\frac{N'}{N}\right)^2\left(\frac{L}{L-\rho}\right)^2\left\{\frac{L-(1+N/N')\rho}{L}\right\}. \qquad \ldots\ldots(5)$$

Retardation of the "R-plate"

$$=\frac{-(N'-N)h'^4}{8R^3}\left(\frac{N'}{N}\right)^2\left(\frac{L-d}{R+d-L}\right)^2\left\{\frac{L-d-(1+N/N')R}{L-d}\right\}. \qquad \ldots\ldots(6)$$

To create a star-space in which to construct the see-saw diagram, take a "perfect" anastigmat of unit focal length and place its posterior focus at the intermediate image. Its anterior focus forms the zero for position measurements in star-space: distances will be counted positive to the right of this point. This artificial star-space has index N'.

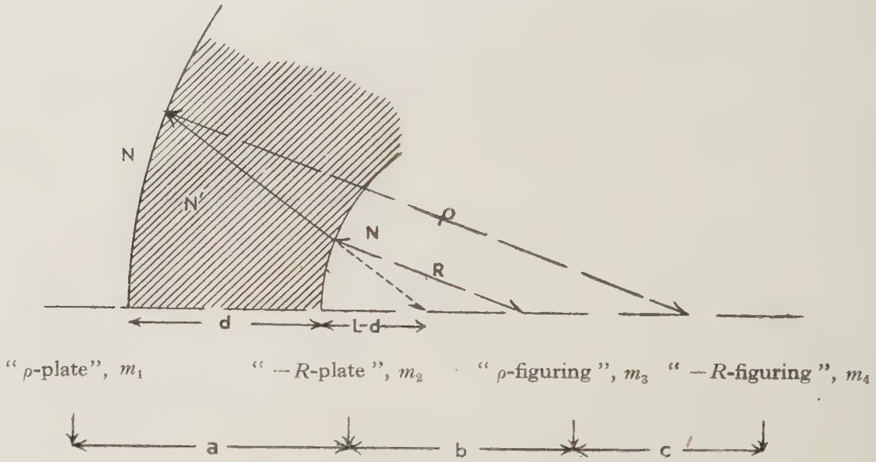

Figure 1. Two-surface system and its see-saw diagram.

Figure 1 shows the two-surface system and its see-saw diagram. Imaging the surfaces and paracentres of the two aspheres into star-space gives immediately:

$$a=\frac{-1}{R+d-L}+\frac{1}{\rho-L},\ =\frac{R+d-\rho}{(\rho-L)(R+d-L)};\quad a+b=\frac{1}{L}+\frac{1}{\rho-L},\ =\frac{\rho}{L(\rho-L)}:$$

$$b=\frac{1}{L}+\frac{1}{R+d-L},\ =\frac{R+d}{L(R+d-L)};$$

$$b+c=\frac{1}{L-d}+\frac{1}{R+d-L},\ =\frac{R}{(L-d)(R+d-L)};$$

$$=\frac{1}{L-d}-\frac{1}{L},\ =\frac{d}{L(L-d)};\quad a+b+c=\frac{1}{L-d}+\frac{1}{\rho-L},\ =\frac{\rho-d}{(L-d)(\rho-L)}.$$

$\ldots\ldots(7)$

The process of imaging the various plates into star-space alters their strengths, multiplying each by the 4th power of the magnification with which the plate is itself imaged—that is, by the +4th power of the distance of each plate from the posterior focus of the unit-focal-length anastigmat. Thus the strengths of the "ρ-plate" and the '$-R$-plate" are multiplied by $(\rho-L)^4$ and $(R+d-L)^4$, respectively, and the strengths in star-space, taken negatively, since the plates are missing, are

$$\rho\text{-plate}: \; m_1 = H^4\frac{(N'-N)}{8\rho^3}\left(\frac{N'}{N}\right)^2 L^2(L-\rho)^2\left\{\frac{L-(1+N/N')\rho}{L}\right\}, \quad \text{retard lacking;} \quad \ldots\ldots(8)$$

$$-R\text{-plate}: \; m_2 = -H^4\frac{(N'-N)}{8R^3}\left(\frac{N'}{N}\right)^2 (L-d)^2(L-d-R)^2\left\{\frac{L-d-(1+N/N')R}{L-d}\right\}, \quad \text{retard lacking,} \quad \ldots\ldots(9)$$

H being the zonal height in star-space.

Now we can confer on m_3 and m_4—the figurings on the two surfaces—any values we please. The possibility of achieving anastigmatism by figuring therefore reduces to the question of whether the ratio m_1/m_2 has the value demanded by the 4-plate theorem.

From [8] and [9],

$$\frac{m_1}{m_2} = -\frac{R^3}{\rho^3}\left(\frac{L}{L-d}\right)^2\frac{(L-\rho)^2}{(R+d-L)^2}\left\{\frac{1-(1+N/N')\rho/L}{1-(1+N/N')R/(L-d)}\right\}, \quad \ldots\ldots(10)$$

while the ratio $-b(b+c)/(a+b)(a+b+c)$ demanded by the 4-plate theorem (2), is

$$\frac{-R}{\rho}\left(\frac{R+d}{\rho-d}\right)\frac{(L-\rho)^2}{(R+d-L)^2}, \quad \ldots\ldots(11)$$

unless m_1, m_2 are both zero.

Accordingly

$$\left(\frac{L-d}{L}\right)^2 = \frac{R^2}{\rho^2}\left(\frac{\rho-d}{R+d}\right)\left[\frac{1-(1+N/N')\rho/L}{1-(1+N/N')R/(L-d)}\right]. \quad \ldots\ldots(12)$$

§ 7. THE TWO-MIRROR SYSTEM AS A SPECIAL CASE

For a system composed of two mirrors, $N' = -N$, and (12) becomes

$$\frac{L-d}{L} = \frac{R}{\rho}\sqrt{\left(\frac{\rho-d}{R+d}\right)}. \quad \ldots\ldots(13)$$

This, then, is the condition that a two-mirror aplanat should be an anastigmat, and accordingly is the formula we need to design a two-mirror anastigmat micro-objective imaging at a finite "tube-length". When we set the tube-length infinite—i.e., set $L-d=R/2$, (13) reduces to Schwarzschild's result, that the focal length must either be half the distance between the mirrors or infinite. The former alternative has a simple geometric consequence relating to "projection" anastigmat micro-objectives. To make such a reflecting anastigmat, set up a concave mirror and a convex one, so:

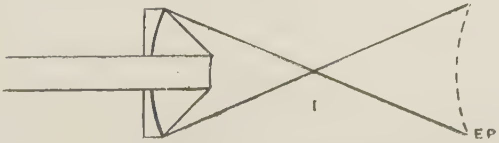

Figure 2. Anastigmatic projection micro-objective.

Adjust the distance between the mirrors until I, the image of the surrounding landscape, lies midway between the concave mirror and EP, the exit pupil of the pair—i.e., the image in the concave of the convex. Then figure the combination aplanatic for that setting, and it will be, *ipso facto*, anastigmatic.

When we take the other alternative—that the focal length should be infinite as well as the tube-length—the system reduces to a pair of coaxal confocal paraboloids, either of Cassegrain or Gregorian arrangement. The anastigmatism of such a system was pointed out by Schwarzschild.

§ 8. ANASTIGMAT SINGLET LENSES

Let us re-write expression (12):

$$\frac{1}{\rho}\left\{\frac{L}{\rho}-\left(\frac{1+N}{N'}\right)\right\}(\rho-d)=\frac{L-d}{R}\left\{\frac{L-d}{R}-\left(\frac{1+N}{N'}\right)\right\}(R+d). \qquad \ldots\ldots(14)$$

As this is quadratic in L, there are in general two object-distances for which a given singlet can be figured anastigmatic. We are specially interested in flat-fielded anastigmats, for which necessarily $R=\rho$, and in objects at infinity, for which $L=\rho N'/(N'-N)=\rho n/(n-1)$ (setting $N'/N=n$).

The condition for anastigmatism then becomes

$$L\left(L-\left(\frac{n+1}{n}\right)\left(\frac{n-1}{n}\right)L\right)(\rho-d)=(L-d)\left(L-d-\left(\frac{n+1}{n}\right)\left(\frac{n-1}{n}\right)L\right)(\rho+d). \qquad \ldots\ldots(15)$$

$$L\frac{L}{n^2}\left(\frac{n-1}{n}\right)(L-d)=(L-d)\left(\frac{L}{n^2}-d\right)\left(\frac{n-1}{n}\right)(L+d), \qquad \ldots\ldots(16)$$

$$\left(\frac{L-d}{L}\right)\left(\frac{L-n^2d}{L}\right)=\frac{(n-1)L-nd}{(n-1)L+nd}, \qquad \ldots\ldots(17)$$

or

$$\left(\frac{n\rho-(n-1)d}{n\rho}\right)\left(\frac{n\rho-(n-1)n^2d}{n\rho}\right)=\frac{\rho-d}{\rho+d}, \qquad \ldots\ldots(18)$$

or setting $x=d\ \rho$,

$$\left(1-\left(\frac{n-1}{n}\right)x\right)\left(1-(n-1)nx\right)=\frac{1-x}{1+x}, \qquad \ldots\ldots(19)$$

from which either $x=0$, and the "lens" vanishes, or

$$(n-1)^2x^2-(n-1)\left(\frac{n+1}{n}\right)x+2-(n-1)\left(n+\frac{1}{n}\right)=0, \qquad \ldots\ldots(20)$$

giving

$$x=\frac{n+1}{2n(n-1)}\pm\frac{1}{2(n-1)}\left[\left(\frac{n-1}{n}\right)^2-4\{1-n(n-1)\}\right]^{\frac{1}{2}}. \qquad \ldots\ldots(21)$$

So two real values of x, $=\frac{d}{\rho}$, exist if $\left(\frac{n-1}{2n}\right)>1-n(n-1)$, i.e., for $x>1{\cdot}602\ldots$

For the critical value, $x=\frac{n+1}{2n(n-1)}$, $\simeq 1{\cdot}35$.

The product of the two roots, $2-(n-1)(n+1/n)$, will be positive for the usual range of n (up to a little beyond $n=1{\cdot}8$), so that both roots will be positive. Now d must be positive for a physically real lens. Therefore no flat-fielded singlet anastigmat taking parallel light on a concave front face exists for the "usual" range of n.

The two kinds of flat-fielded singlet anastigmat taking parallel light on a convex front face assume a specially simple form if we set $n(n-1)=1$, in which case $x_1=d/\rho$, $=1$ or n. When $x=1$ the intermediate image is in the Amici position with respect to the second surface, so that the second missing anastigmatizing plate has zero strength: the first missing anastigmatizing plate lies on the second surface, so that only that surface need be figured. The figuring needed is minus that of the Cartesian hyperboloid of eccentricity n. The value of n in question is $\frac{1}{2}(\sqrt{5}+1)$, $=1{\cdot}618\ldots.$

When $x=n$, the second surface is concentric with the final image, and the second missing anastigmatizing plate then has infinite strength: when this plate is imaged into star-space its strength becomes zero in such a way that its contribution to the astigmatism is finite and annuls that due to the remaining plates. The required figurings are $1/n(=n-1)$ times Cartesian ellipsoidal on the front face, and $-n^2$ times Cartesian hyperboloidal on the rear face. [The Cartesian ellipsoid has eccentricity $1/n$; the hyperboloid eccentricity n.]

(To verify that this does in fact produce anastigmatism set $d=n\rho'-\epsilon$, and then make $\epsilon\to 0$.)

Clearly, as we reduce n from $1{\cdot}618\ldots.$, the difference in thickness of the two kinds of singlet will decrease, attaining zero for the critical value $1{\cdot}602$. Both surfaces in general will have to be figured. Indeed, if we specify that the singlet is to be anastigmatic and flat-fielded, with only one figured surface, since it is then a 3-plate system, the plates, if not zero, must all coincide. It will thus be apparent that—for parallel light incident on the front face—no singlet anastigmat other than the example quoted for $n=1{\cdot}618\ldots.$ is possible having only one figured surface. It is natural to ask: What can be done with indices less than the critical $1{\cdot}602\ldots.$? If, for example, we demand a flat field, how much will the residual astigmatism be? It is easy to see that it will be negative in sign if the Petzval sum is made zero, for when the second surface is placed in the Amici position with respect to the intermediate image, the missing plate of the first lies between the two surfaces. To produce aplanatism both surfaces must therefore have negative spherical aberration figurings: take the missing plate as pivot-point, and it is clear that the astigmatism is negative. But as we know it cannot be zero for any value of d, under the other conditions imposed, it must be negative for every value of d. If, on the other hand, we so choose the radius of the second surface that when placed at the paracentre of the first it is also in the Amici position with respect to the intermediate image, we can anastigmatize the system by figuring the second surface alone, and (if $n<1{\cdot}618\ldots.$) the field curvature due to non-zero Petzval sum is then positive. With suitable intermediate values of d and R, then, it must be possible to produce flat-fieldedness, and—since we have two variables, d and R, at our disposal—we may expect to achieve that flat-fieldedness by figuring one surface only.

I have, in fact, made the analysis, and hope to present it in a subsequent paper.

§ 9. DISTORTION OF ANASTIGMATIC SINGLETS

For the sake of completeness let us calculate the distortion of the two forms taken by the flat-fielded anastigmatic singlet when $n=1{\cdot}618\ldots.$ In the case $\rho=R=d$, having only one figured surface, it will obviously conduce to simplicity if we place the diaphragm—the position of which does not affect the distortion—at the pole of the figured surface. As this point is also the centre of the convex surface, no distortion will arise at the convex surface, and the distortion produced by the concave surface will be simply that arising from refraction out of glass. Thus, if we define the fractional distortion as

$$\frac{\text{size of actual image} - \text{size of ``ideal'' image}}{\text{size of ideal image}}$$

for a given principal ray, making, say, θ with the axis in "fresh air". the fractional distortion will be

$$\frac{\tan\sin^{-1}(n\sin\theta)}{n\tan\theta}-1,\ =[1-n^2\sin^2\theta]^{-\frac{1}{2}}[1-\sin^2\theta]^{\frac{1}{2}}-1, \qquad \ldots\ldots(22)$$

so that this lens has a "pin-cushion type" distortion of amount

$$\tfrac{1}{2}(n^2-1)\,\theta^2,\ = {\cdot}809\ldots.\,\theta^2 \qquad \ldots\ldots(23)$$

in the extended paraxial region.

In the more general case, if we diaphragm at the pole of the R-surface, three further contributions will be involved: that due to the projection on a plane of the curved image-surface formed in glass by the ρ-surface; that due to the missing anastigmatizing plate of the ρ-surface; and that due to the figuring on the ρ-surface. The "projection" (Petzval) contribution is in no way affected by the presence of asphericities, and it will suffice here to give without derivation the formula for its magnitude: if the see-saw diagram be constructed in a star-space of index N_s and if θ_s be the angle made by the principal ray with the axis, and Δ_s the distance of a refracting centre "down-light" of the pivot-point, then the "Petzval" contribution produced by refraction out of an ocean of N into the sphere of radius ρ, index N', struck on the centre in question, is

$$-\left(\frac{N'-N}{2NN'}\right)\cdot\frac{\Delta_s}{\rho}\,\theta_s^2\,.\,N_s. \qquad \ldots\ldots(24)$$

The fractional distortion produced by a plate of retardation KH^4 placed D down-light of the pivot-point in the same diagram is easily seen to be

$$\frac{+4KD^3\,\theta_s^2}{N_s}. \qquad \ldots\ldots(25)$$

If, then, we construct in "fresh air", i.e. in air in front of the front face of the lens, the see-saw diagram for the case $R=\rho$; $d=n\rho$; $n=1+1/n,=1{\cdot}618\ldots.$ and set the pivot-point at the R-pole, which is distant $n^2\rho$ behind the ρ-pole in fresh air, and $n\rho$ behind the ρ-centre, the Petzval contribution of the front surface reduces to

$$\tfrac{1}{2}(n-1)\,\theta^2, \qquad \ldots\ldots(26)$$

the missing anastigmatizing plate has strength $\frac{H^4}{8\rho^3 n^3}$ retardation lacking, and the strength of the figuring on the ρ-surface is $\frac{+H^4}{8\rho^3 n^4}$ retardation, so that the contributions to distortion from these two plates are

$$\theta^2/2 \text{ and } -n^2\theta^2/2 \text{ respectively.} \qquad \ldots\ldots(27)$$

There remains the distortion due to refraction out of glass at the R-pole. The principal ray in glass makes not, as in (23), θ with the axis, but $n\theta$, so that the contribution due to the R-surface is

$$\tfrac{1}{2}(n^2-1)n^2\theta^2. \qquad \ldots\ldots(28)$$

Adding these four contributions, and remembering that $n-1=1/n$, we obtain for the pin-cushion type distortion of this lens the value

$$\tfrac{1}{2}(n^3-1)\theta^2, \ = 2{\cdot}118\,\theta^2 \qquad \ldots\ldots(29)$$

—a value so great that I can hardly believe this singlet will find application.

[It is in general possible, if not preferable, to include the R-surface in the see-saw diagram in the determination of distortion. But as in this case the surface is paracentred at the image, its plate lies at infinity in the diagram, so that it would have been necessary to take the limit of the sum of the "plate" and "Petzval" terms. This, in effect, had already been done in (23).]

§ 10. ACHROMATIZATION OF FLAT-FIELDED ANASTIGMAT SINGLETS FOR IMAGE SIZE

It is in principle possible to achromatize the anastigmats of § 8 for image size by a suitably placed diaphragm. Let us consider the problem in quite general terms. Let the final image be formed at a distance u_2 for "red"; $u_2+\delta u_2$ for "blue" behind the pole of the $-R$-surface: let the focal lengths be f and $f+\delta f$ respectively. An object at infinity θ off axis is imaged paraxially at height θf for red and $\theta(f+\delta f)$ for blue. Let the ray passing through both these image-points hit the axis in image-space D "upstream" of the red image. Then we shall have

$$D=f\cdot\frac{\partial u_2}{\partial n}\cdot\frac{\partial n}{\partial f}, \qquad \ldots\ldots(30)$$

and if the system is diaphragmed at this point, in image-space, the principal ray of either colour will go through both images. Whether the system be anastigmatic or no, when the object is at infinity we shall have

$$u_2=\frac{R[\rho-(n-1)d/n]}{(n-1)[R-\rho+(n-1)d/n]} \qquad \ldots\ldots(31)$$

and

$$f=\frac{R\rho}{(n-1)[R-\rho+(n-1)d/n]}. \qquad \ldots\ldots(32)$$

This gives

$$D=u_2+\frac{Rd}{n^2[R-\rho+d-d/n^2]}, \qquad \ldots\ldots(33)$$

—which is, in the case of our flat-fielded anastigmats and approximations thereto in which $R+d>\rho+d/n^2$, a virtual diaphragm position in image-space. It images into glass at a distance D_g upstream of the $-R$-pole given by

$$D_g=\frac{Rd}{n[R-\rho+(n-1)d/n]}, \qquad \ldots\ldots(34)$$

which in the cases of principal interest to us is again a virtual image. Fortunately it images through the front (ρ) surface into what is usually a real image in "fresh air"—i.e., in air in front of the front face of the lens.

The achromatizing point in fresh air, then, lies in front of the ρ-pole by a distance

$$D_a=\frac{\rho d[(n-1)(R+d)-n\rho]}{[n\rho-(n-1)d]^2-R[n^2\rho-(n-1)^2d]}. \qquad \ldots\ldots(35)$$

In the flat-fielded anastigmats $R=\rho$, so that

$$D_a=\frac{\rho[\rho-(n-1)d]}{(n-1)[n\rho-(n-1)d]}. \qquad \ldots\ldots(36)$$

In the special case $n=1/(n-1)=1{\cdot}618\ldots$ we had $d=\rho$ or $n\rho$. When $d=\rho$, $D_a=\rho/2n$, so that this lens has its paraxial achromatizing point $0{\cdot}309\rho$ in front of the front surface. When $d=n\rho$, $D_a=0$: the achromatizing point lies at the pole of the front surface.

Paraxial transverse achromatism, like extended paraxial anastigmatism, represents only an approximation to optimal correction of the types of error in question.

Determination of the residuals, however, lies outside the scope of the present method, in which our purpose is not to elicit a final design, but rather to elucidate leading features which these simple anastigmats must possess.

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CECIL REGINALD BURCH,
Nineteenth Duddell Medallist.

At the meeting of the Society on 9 April 1943 the nineteenth Duddell Medal was presented to Dr. C. R. Burch. The Medal is awarded to " persons who have contributed to the advancement of knowledge by the invention or design of scientific instruments, or by the discovery of materials used in their construction ".

As may be seen in some of the work of the two previous Duddell medallists—Lawrence's cyclotron and Coolidge's multi-sectional high-voltage X-ray tubes—one of the outstanding characteristics of modern physics is the large-scale application of vacuum technique. Burch's name is closely associated with the development of modern high-vacuum equipment, and it was primarily in recognition of his contributions to this subject that the present award was made. His invention of the oil-diffusion pump, his still for the production of vacuum oils, greases and waxes, and his development of demountable vacuum-tight joints have made possible the attainment of high vacua on an engineering scale and thus greatly increased the range of experimental investigation. It was Burch's work on the production of flat surfaces for vacuum work that re-aroused his old interest in the grinding of optical surfaces and the possibilities of new developments in optical technique, on which he is now engaged. His remarkable versatility is seen also in his pioneer work in the invention (with Ryland Davis) of the induction furnace.

Burch was born in 1901 and was educated at Oundle School and Gonville and Caius College, Cambridge. After taking the Natural Sciences Tripos he was for the next ten years with the Metropolitan-Vickers Electrical Co., Ltd. His interest in optics led him to accept a Leverhulme Fellowship in 1933, so that he could devote the whole of his time to optical work, which he carried out at the Imperial College, London. In 1935 he moved to the University of Bristol, where he is carrying on this work. Dr. Burch is not only a first-class physicist and instrument designer, but in addition possesses considerable mathematical ability and mechanical skill, which he uses to construct, for the most part, his own instruments and apparatus. In him Duddell would have recognized a kindred spirit.

An abridged version of the address " A Technologist looks at the Future ", which Dr. Burch delivered on the occasion of the presentation of the Medal, appears in *Nature*, vol. 152, p. 523 (6 November 1943).

PROFESSOR (now BRIGADIER) BASIL F. J. SCHONLAND, O.B.E., M.A., PH.D., F.R.S., Second (1943) Charles Chree Medallist.

THUNDERSTORMS AND THEIR ELECTRICAL EFFECTS

By B. F. J. SCHONLAND, O.B.E., M.A., Ph.D., F.R.S.

Second Charles Chree Address, delivered 16 *July* 1943

§1. INTRODUCTION

I am extremely sensible of the honour paid me by the Physical Society in the award of the Charles Chree Medal. I appreciate it the more because this recognition of the work done in South Africa will give pleasure and encouragement to those who have worked so hard and enthusiastically with me for many years past and to those who have supported our work.

§2. THE INFLUENCE OF CHARLES CHREE ON THE DEVELOPMENT OF ATMOSPHERIC ELECTRICITY

In the first Charles Chree Address, delivered before you in 1941, Professor S. Chapman gave an account of the achievements and influence of Charles Chree in the field of geomagnetism. I should like to add a few words concerning Chree's important influence upon the study of atmospheric electricity.

Chree's published contributions to this subject were marked by the keen critical judgement he brought to bear on all he touched. Much of the mechanism of the electrification of the atmosphere lies outside the reach of direct experiment, and deductions made from measurements at the earth's surface require careful scrutiny. Chree's attitude of cautious and critical enquiry and his rooted objection to speculations as to the cause of the relationships he was studying have been important factors in shaping the judgement of other workers in the same subject and restricting speculation to the data in hand.

Speculation on ultimate causes with a minimum of facts to guide and check the imagination is a privilege which may be granted to genius. Chree, in his Presidential Address to this Society in 1908, called it "scientific poetry", and allowed it to minds like Lord Kelvin's. It is wiser for more ordinary mortals to wait until the full facts have been collected. For the geophysicist, who may have to wait one or two years for another earthquake, magnetic storm or thunderstorm, this is a severe but necessary discipline.

The literature of thunderstorm electrification contains a great deal of the unrestricted speculation to which Chree objected. The facts have proved exceptionally difficult to obtain, and "scientific poetry" has had a long reign. But in the course of the last thirty years, hard work and ingenious experimenting —largely done and inspired by workers in this country—have yielded a rich harvest of accurate information.

I shall not attempt in this address any general survey of this achievement. In spite of all that has been done, the facts are still deficient in certain directions. This and the circumstances of the time have prevented me from undertaking the difficult task of a critical survey.

I shall, therefore, concern myself in the main with some of the contributions made by workers in South Africa to the gathering of the facts. Such an account will, perhaps, be a not unsuitable commemoration of the work of Charles Chree, whose life was largely spent in the collection and critical analysis of geophysical data.

§3. THE ELECTRICAL POLARITY OF THUNDERCLOUDS

The beginning of our modern knowledge of thunderstorm electrification was marked by an important controversy over a plain but difficult question of fact, the polarity of thunderclouds. C. T. R. Wilson, as a result of his classic studies of fields and field-changes at the earth's surface, produced much evidence for the view that thunderstorms are bi-polar, with the positive charge elevated above the negative. His conclusions were supported and extended by observations on thunderstorms in South Africa. Sir George Simpson, on the other hand, held that the polarity was the reverse, basing his arguments upon extensive measurements of the charge on rain and a different interpretation of the field-change results. The controversy has been settled by the recent remarkable experiments carried out at Kew by Simpson, Scrase and Robinson, in which balloons with equipment to record the electric fields encountered have been flown below, above and through a number of thunderclouds.

It is now, I think, accepted that the top of a thundercloud is practically always positively charged, and that the base is negative. The Kew observations indicate that a pocket of positive charge is usually enclosed in this base. The existence of this additional lower positive charge is not yet directly established for storms in tropical and sub-tropical latitudes. Aside from this positive pocket there is, however, likely to be general agreement, from the results of this work and much else, that the thunderstorm electrical generator is bi-polar and shows a remarkable uniformity in its polarity all over the world.

§4. THE ELECTRIC CHARGE ON THE EARTH

This conclusion as to cloud polarity, apart from its application to the problem of the mechanism of generation of charge, has several important consequences.

In the first place, the majority of electrical discharges to ground, since they come from the base of the cloud, must convey negative electricity to the earth. The deduction is of interest and value to electrical engineers, and is confirmed by many direct measurements of the current in strikes to power lines and tall structures in various parts of the world.

Secondly, the conduction currents which flow in thunderstorm fields between the earth and the cloud and between the cloud and the ionosphere must, in the main, carry positive charge away from the earth and into the conducting regions of the ionosphere.

These two effects, the direct discharge and the conduction current, evidently play an important part in charging the earth with negative electricity. There is good reason to believe, as Wilson first suggested, that they provide the hitherto unknown mechanism for the maintenance of the earth's negative electric charge. The 1800 or so storms in action over the earth's surface at any one time can certainly provide the 1000 amperes required to compensate for the continuous loss of charge from earth to upper air by conduction in regions of fine weather, with something over to compensate for the predominating positive charge on rain. Moreover, the diurnal variation in thunderstorm activity as the land regions of the earth pass under and away from the sun, as worked out by F. J. Whipple, shows a very striking correlation in time and magnitude with the diurnal variation of the earth's charge.

§5. THE BRANCHING OF LIGHTNING DISCHARGES AND LONG SPARKS

In the course of these studies on the polarity of thunderclouds there arose a new and unexpected problem. My observations of field changes known to be produced by flashes to ground showed that these flashes carried negative electricity to the earth and so, presumably, started from the negatively charged portion of the cloud. Simultaneous photography and observation of the flashes themselves showed that they were branched towards the earth in the familiar way.

But from these results it then followed that the lightning flash to ground branched away from the negative electrode and behaved in a manner which was the complete reverse of the branching in the usual laboratory spark discharge, for this had always been observed to branch away from the positive electrode.

It was therefore clear that the "negative branching" found in the lightning flash must arise from special conditions which are not usually present in laboratory discharges. To reproduce these conditions it would be necessary to reproduce the extensive point-discharge current flowing up to the cloud from the positively charged earth, and thus to provide a positive ion space-charge. This should create a situation which would lead to branching from the negative or cloud electrode into the charge in the air.

An experiment on these lines was arranged by Allibone and myself. The negative electrode was a pointed rod and the positive electrode an earthed plate studded with nails. The discharge was produced by a surge-generator giving a million volts. It gave the effect we had hoped for, and the resulting artificial flash was extensively branched away from the negative pole (figure 1).

This experiment not only cleared up the branching problem, it suggested that much might be learned about processes in the spark discharge from a study of the large-scale discharge of lightning.

§6. THE PROGRESSIVE DEVELOPMENT OF THE LIGHTNING DISCHARGE

To learn more about the lightning discharge it was clearly necessary to study its method of progression. On looking through the literature, I learned that an instrument for this purpose already existed. The simple but extremely ingenious device which is known as the Boys camera was invented and made by Sir Charles Boys about the year 1900. He had not had an opportunity of securing satisfactory pictures with it when he first described it in 1926, and added the characteristic remark that "a little information can do no harm".

The principles which this camera employs are now well known. If a photograph of a flash is taken by a single camera in which there is relative motion between lens and film, the image is distorted because points along the path which become luminous later than others will be shifted further from their correct positions. But if the progress of the luminosity is very fast, the distortion is small, and it is difficult to detect it by comparison of the record with that from a separate fixed-lens camera. Boys therefore arranged to compare two images which were photographed on the same film by lenses moving in opposite directions. This he achieved by the ingenious device of placing the lenses at opposite ends of the diameter of a circle. In a later form of his camera the lenses were fixed and the film moved on the inside of a circular drum, the images being formed at opposite ends of a diameter by means of up-and-down reflections from right-angle prisms.

If these two distorted pictures are properly mounted side by side, corresponding points will be separated by a distance $k+2vt$, where t is measured from the start of the luminosity of the discharge, v is the relative velocity of lens and film, and k a constant. With simple equipment and values of v below 100 miles an hour, t can be obtained to the nearest microsecond. This means that even if the discharge travelled with the speed of light, its progression could be distinguished over a path length of 333 metres.

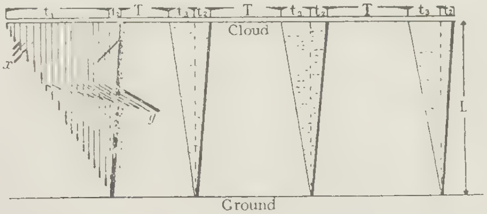
The great merit of the Boys camera, particularly in its original form, in which the two lenses are rotated by a hand-driven flywheel, is its simplicity. This makes it possible to use it quickly in the open country and to change plates and films rapidly. Three cameras of this kind were made by us in South Africa in 1932, under the direction of a Committee of the South African Institute of Electrical Engineers. We paid careful attention to lens-matching and mechanical construction, and I attached to the equipment a small slow-moving camera with a rotating lens to record the separate strokes of which most flashes are composed. The high speed of the Boys camera itself causes consecutive strokes to be recorded over many revolutions of the lens-system, and out of their proper order, so that their succession cannot be determined without such an additional camera.

Most of the records were taken in the neighbourhood of Johannesburg. Although this is a region abounding in summer thunderstorms, a great deal of work was required to secure a reasonable number of good results. Each picture had to be taken at night in the open country, away from the lights of the town, and at a suitable point in front of the storm. Rain often spoiled the clarity of the pictures, and many fruitless journeys were made, but several hundred flashes were recorded in the course of four years from 1932 to 1936. Most of these were taken by Mr. F. Collens, a Johannesburg engineer, and by Dr. D. K. Malan, of the University of Cape Town, who also gave me most valued assistance in the measurement and interpretation of the records.

To the Boys camera we added a van fitted with cathode-ray oscillograph equipment for the simultaneous recording of the electrical field-changes taking place during the discharge. This equipment was constructed by Prof. D. B. Hodges, of the Natal University College, and myself on the lines developed in this country by Watson-Watt and Appleton.

Most lightning discharges are intermittent in character and consist of separate strokes occurring at intervals of the order of one-tenth of a second. These, I have suggested, probably arise from the successive junction and discharge of different generating centres to the conducting channel provided by the first stroke to ground. Observations show that discharges from isolated storms of the thermal convection type have fewer strokes (often only one) than those from frontal storms, when the number may be as great as forty. It can be shown that the intervals between strokes correspond to the times required to join centres some 5 km. apart.

The Boys camera pictures show that each of these strokes is itself double, and begins with a rather feebly luminous downward-moving leader streamer, travelling with an effective velocity of about 150 km./sec. if it is the first leader, and about 2000 km./sec. in subsequent strokes. At or near the ground this gives rise to a brilliant upward-moving return streamer, which moves with a velocity of some 30,000 km./sec., though in its earliest stages it can attain 150,000 km./sec., half the speed of light. These results are illustrated in figures 2, 3 and 4.



–Average values: t_1 . . 0·01 sec; t_3 . . 0·001 sec; t_2 . . 0·00004 sec; T . . 0·03 sec; L . . 2·0 km.

Figure 2. Diagrammatic representation of the processes shown by a Boys camera. Vertical discharge channel ; linear relative motion of lens and film.

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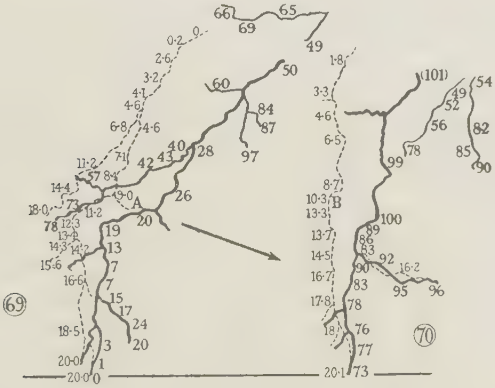

Figure 3. Time-table of leader (in milliseconds) and return (in microseconds) processes of the two nearly simultaneous discharges shown in figure 4.

(*Reproduced by courtesy of the Council of the Royal Society.*)

The electrical observations made simultaneously with the camera pictures show that a considerable negative charge is lowered into the air as a charge on the leader and its branches. What remains of this after neutralization of the positive space-charge is rapidly passed to the ground during the return part of the stroke. They also show, what the camera cannot, that the leader process starts higher up than the base of the cloud, and that a further discharge of longer duration (the "c" change of Appleton and Chapman) takes place along the channel after the return stroke has entered the cloud. This removes what electricity remains in the generating centre. I have found that with few exceptions all the separate strokes start from the negative charge in the cloud and convey negative electricity to earth.

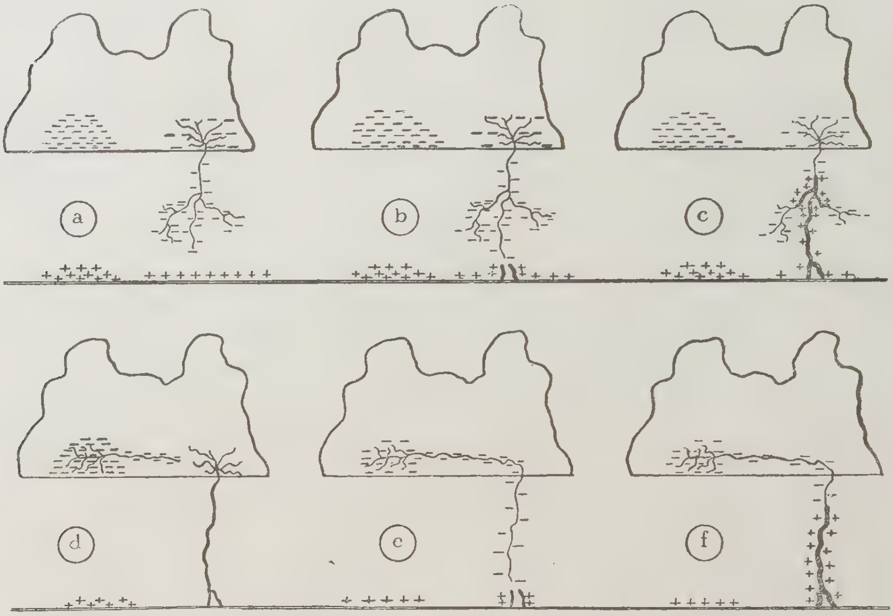

Figure 5. The electrical processes in a multiple discharge.
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A diagrammatic representation of the electrical processes in a multiple discharge is given in figure 5. The joining up of new cloud charge-centres to the original channel may, as Bruce and Golde have recently pointed out, take place from the channel outwards instead of in the direction here shown.

It should be mentioned that observations by McEachron and others in the United States have shown that very high buildings can sometimes give rise to a positive leader streamer proceeding from ground to cloud. This is not followed by a return streamer.

§7. THE FIRST LEADER PROCESS

The mode of progression of the first leader-streamer is extremely interesting. This is the only process in the lightning discharge which is not recorded by the camera as a continously moving luminous streamer. What is recorded as the first leader-streamer proceeds from the cloud is a series of steps (figure 6). These steps vary in length from 15 to 36 metres, and after each step there is an apparent pause lasting from 16 to 100 microseconds. The photographic

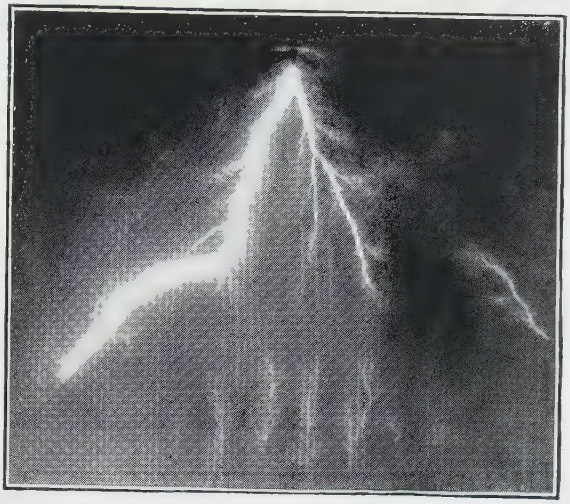

Figure 1.

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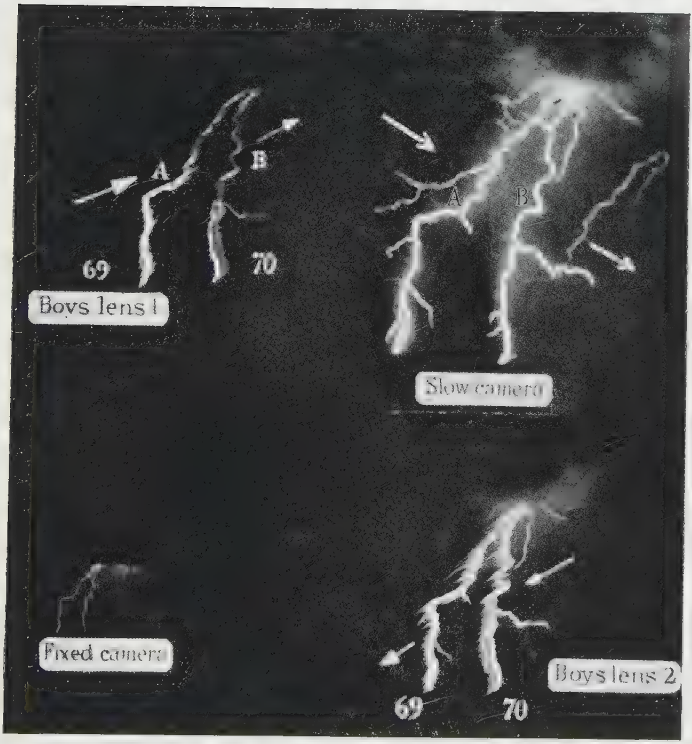

Figure 4.

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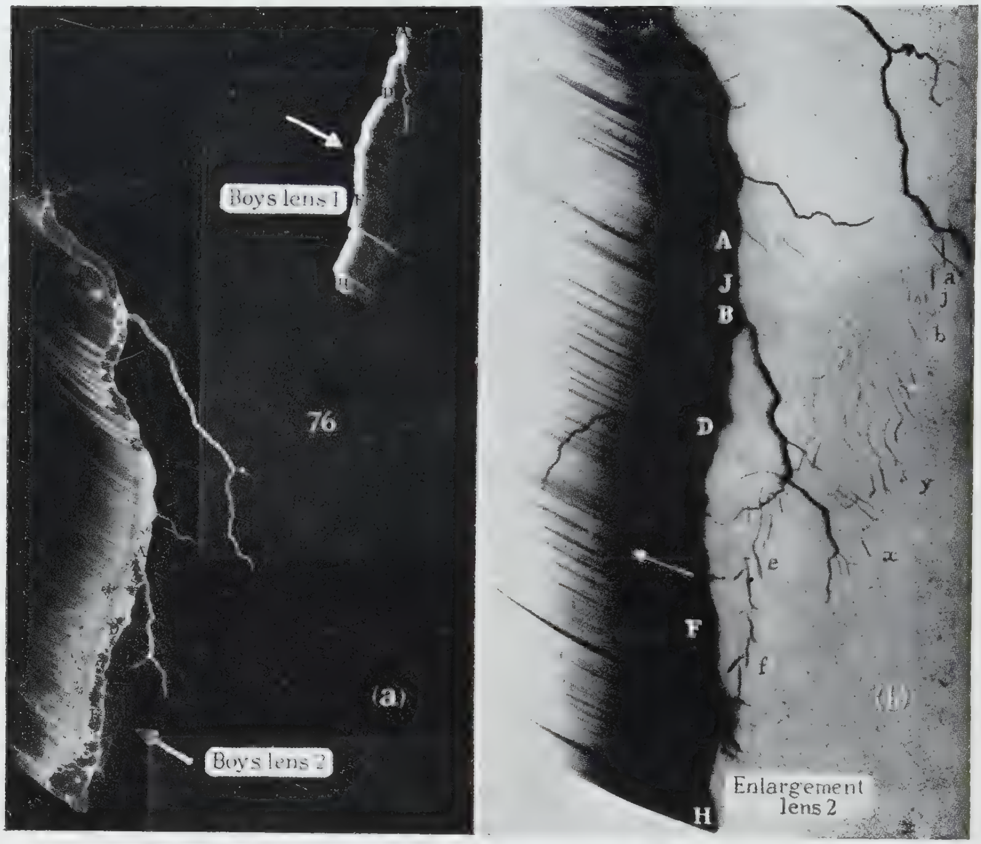

Figure 6.

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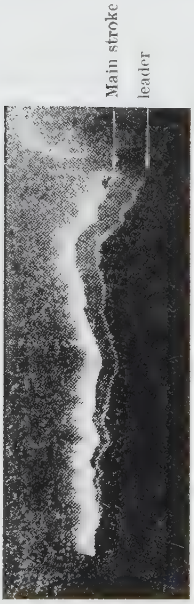

Figure 7.

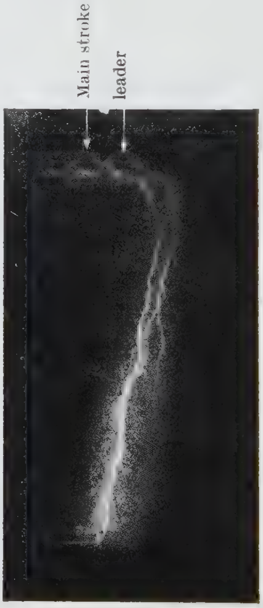

Figure 8.

(*Reproduced by courtesy of the Council of the Institution of Electrical Engineers.*)

record thus shows an intermittently extending luminosity which is brightest over the portion which is new, and for a short way back along the previously formed channel. In most of our photographs this leader is shown only as a series of discrete steps, often very difficult to distinguish. The slower leaders show the shorter steps, and the pauses between them are also shorter. For all but 10% of the leaders, however, the duration of the apparent pause is between 50 and 90 microseconds. This extremely narrow range of variation in duration makes it quite certain that any explanation of the pause must involve a gas-discharge process of a fundamental character.

The steps themselves are produced by a very rapid streamer movement whose velocity is of the order of 50,000 km./sec., but the whole process, owing to the pauses between stages, is the slowest in the discharge and takes about 0·01 second to reach the ground. Its effective velocity is usually 150 km./sec., and never falls below 100 km./sec. This minimum value for the effective velocity of the first leader has an important physical significance, for a velocity of 100 km./sec. is about the minimum velocity at which an electron can move in ordinary air and still create fresh electrons by ionizing collisions. In other words, it is the minimum velocity of an electron avalanche.

Since we are here dealing with a negatively charged streamer, its advance must be due to the continual formation of electron avalanches in front of its tip. The minimum effective velocity of the first leader is so close to the minimum avalanche velocity that it must represent the velocity of a real but invisible "pilot" streamer upon whose channel the visible steps are superimposed. I was thus led to the view that in the first leader we have to deal with the steady and continuous advance of a weakly luminous pilot streamer whose ionized stem becomes unstable as it advances. After a time there occurs a rapid reorganization of the stem, by which it is rendered luminous and highly conducting, and a new step-streamer process runs rapidly down to catch up with the end of the slower pilot streamer. This whole series of events repeats itself as the pilot streamer gradually lengthens. The camera does not record the steady advance of the first stage, and the time spent upon it is the apparent pause, of some 50 μsec. duration. It is a process in which the pilot streamer plays the part of an invisible tortoise and the step streamer that of a visible hare.

This is, of course, little more than a description of the stepped leader process in terms which will lead to a discussion of the physical principles involved. Both the origin of the instability in the stem of the pilot streamer and the nature of the reorganization which is necessary after it has travelled for an average distance of 25 metres offer problems of considerable interest. I have suggested that the instability may arise from the gradual decrease of conductivity of the weakly ionized pilot-streamer stem owing to loss of free electrons by capture and recombination.

This view has been extended and developed in quantitative form by Meek and by Loeb, who consider that it can give a satisfactory explanation of the apparent pause time of 50 μsec. before the instability leads to reorganization. Another explanation, put forward by Bruce, suggests that the instability arises from the increase of current in the stem required to meet increasing losses from corona discharge as the channel lengthens. This effect, he considers, would cause an abrupt transition from glow to arc conditions in the channel when the length of the pilot streamer is about the critical value which is observed.

The final explanation must, I think, wait upon further experiment. We are far from a full understanding of the processes involved in streamer formation or of those which govern the equilibrium in the plasma of the streamer stem. In particular, we require information about the diameter of the pilot-streamer channel and the current in it. Much of this work can be done in the laboratory, for it is now established that pilot streamers of the same nature as are found in lightning precede the breakdown of the electric spark. They were first photographed in the long spark by Allibone and myself, with a rotating film camera, and their further study by Allibone and his associates has produced much information. We now know, from this and other work, that the process of spark breakdown is not that pictured by the classical theory of Townsend. Except for very small gap-lengths, less than 1/76 cm. at atmospheric pressure, breakdown always arises from the initial bridging of the gap by pilot streamers from both electrodes.

In the laboratory spark discharge the positive streamer is usually the most prominent bridging agent, because it develops at lower field strengths than the negative one. This is the reason for the prevalence of positive branching in the laboratory spark, to which I have already referred. In the majority of lightning discharges, however, it is the negative streamer which is responsible for the initial bridging of the gap between cloud and ground. Examples of the two types of leader are shown in figures 7 and 8, the latter from a paper by Allibone on the laboratory spark and the former from one of my own lightning records.

§8. SUBSEQUENT STROKE LEADERS

In the lightning discharge the slow and hesitant pilot streamer is the only process which requires the advance of a streamer into virgin air. As it travels downwards it zigzags and branches to form the characteristic tortuous track followed by subsequent processes, all of which generally travel along the same path. The step streamer which follows and catches up with the pilot streamer and the return streamer which runs up the channel from ground to cloud travel along this pre-ionized path. So also do the leaders to subsequent strokes and their return streamers. All these have two observed characteristics in common: their motion is continuous and their velocity is high. It is, in fact, much too high to be explicable in the same way as the pilot streamer, in terms of self-progression by the formation of electron avalanches in front of the tip, either moving outwards when the tip is negative or inwards when it is positive.

A satisfactory explanation of this rapid advance along a pre-ionized channel has been given by Loeb and Cravath. In general terms, their mechanism calls into play the electrons in the old channel a short distance in front of the streamer tip, and requires them to move a small distance sufficient to produce fresh ions by collision. Such a process can have the high velocities observed, but I have shown that it will travel with reduced velocity if the electrons in the pre-ionized channel have been reduced in number by recombination and attachment to molecules. If d is the distance in front of the streamer tip over which the field strength is great enough for ionization by collision, n the remaining electron density in this region, and $\bar{v}$ the mean electron-drift velocity, the velocity of advance V should be given by an expression of the form

$$V = n^{1/3} \times \bar{v} \times d.$$

We should thus expect that the velocity of advance of a streamer of this kind would decrease with the time that has elapsed since the track it is following was first ionized.

Examination of the various processes in the lightning discharge which follow previously ionized channels confirm this prediction very well. Thus, the return streamer is found to slow up as its tip passes from the fresh ionization in the leader channel near the ground to the older ionization higher up. Again those leader processes which precede strokes subsequent to the first stroke, travel very rapidly and continuously, but with a velocity which is lower the greater the interval which has elapsed since the last stroke.

A careful study of the luminosity in the return streamer shows (see figure 4) that as each branch is reached on the upward journey, an abrupt increase in channel luminosity occurs. One explanation of this is that the streamer is travelling faster as it moves outwards along the more recently ionized branches.

§9. ELECTRICAL EFFECTS ASSOCIATED WITH THE STREAMERS IN THE LIGHTNING DISCHARGE

As would be expected from the analysis of the Boys camera observations, the electrical field-change due to a stroke divides into three stages: the leader or "a" change; the return stroke or "b" change; and the "c" change associated with the final discharge of the electricity left in the cloud centre. A complete flash may show a series of such field-changes corresponding to a series of strokes.

The analysis of these changes, first made by Appleton and Chapman, is complicated by the superposition of electrostatic, induction and radiation effects represented by the three terms on the right of the equation

$$E = \frac{M}{r^3} + \frac{1}{Cr^2} \cdot \frac{dM}{dt} + \frac{1}{C^2 r} \cdot \frac{d^2M}{dt^2},$$

where E is the instantaneous value of the field, r the distance, and M the electric moment of the composite dipole formed by the electric charges and their images in the ground. The first term is the most important in the immediate neighbourhood of the discharge and the last the most important at a considerable distance from it.

A study of the electrostatic term suggests that the first leader process lowers into the air about half the charge on the first cloud centre. This is utilized in the neutralization of some of the positive space-charge and in charging up the leader channel and its branches. The channel charge is removed in the "b" process.

Much useful work remains to be done on these questions by further simultaneous recording of the luminous and the electrical effects of the discharge. The distribution of the space-charge is so far a matter of inference and it is possible, as Bruce and Golde have suggested and the balloon experiments at Kew indicate, that the field below a cloud prior to a discharge is much lower than has been supposed. If this is the case, the distribution of the positive upward-moving ions will be affected by converging winds blowing upwards into the centres of electrification, for these winds will control their path. The concentration of positive space-charge in this way may provide a strong local field between the negatively charged cloud-base and the wind-borne space-charge, and this may be sufficient to trigger off the discharge to ground.

Since radiation effects arise from the term in d^2M/dt^2 they are most prominently shown in the sudden surges in the steps of the first leader and by the great surge which takes place at the start of the return stroke. Both these effects are characteristic of all field-change records at distances exceeding a few miles from the discharge. Examples of field changes from fairly near flashes are shown in figure 9. The small dip about 0·6 milliseconds after the main change is an ionospheric effect, probably a reflection.

The electrical pulses from the first leader process have been used for the "anticipatory triggering" of various recording devices. This is particularly valuable in the photography of lightning with moving-film cameras, since it enables photographs to be taken in daylight. The leader pulses are picked up on a suitable tuned circuit whose response triggers a thyratron and thus operates a fast relay which opens the camera shutter. This can be done well within the 1/100 second occupied by the first leader. The latter part of this leader, together with all the subsequent processes in the discharge, can thus be photographed in daylight. We have employed a camera with a fixed lens and a film of length 2 metres carried on a fast-moving drum. With a total exposure of half a second, the full details of the majority of lightning flashes can be recorded in daylight without serious fogging of the film.

§ 10. THE WAVE-FORM OF ATMOSPHERICS

With this knowledge of the electrical processes in lightning discharges, it is interesting to examine the wave-form of the atmospherics produced by them. The atmospheric from a discharge to ground can generally be distinguished from one due to a discharge in the cloud by the long series of preliminary pulses corresponding to the surges in the stepped-leader process. These leader pulses have been recorded at distances exceeding 3000 km. from the parent flash. Anticipatory triggering of high-speed cathode-ray oscillographs can therefore conveniently be used to obtain detailed records of the wave-form of the main disturbance which follows them.

When these main field changes are examined they are found to be considerably more complex than would be expected. The sudden rise of current in the return stroke should produce a radiation field consisting of a single complete oscillation. Nothing so simple is observed; by day we find, as did Appleton and Watson-Watt and their associates in extensive pioneer investigations made in this country, a series of some five oscillations like a damped train of waves, whose period lengthens along the train and is also a function of the distance from the parent flash. By night we find a similar form at great distances, but the nearer records show a series of separate oscillations with quiet intervals

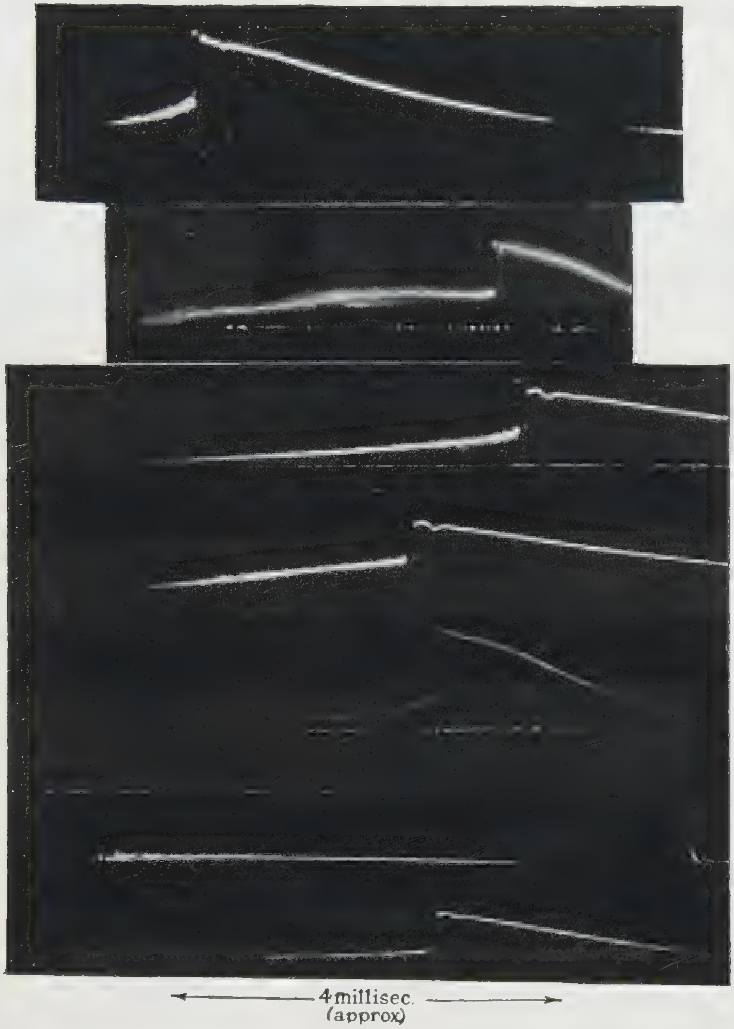

Figure 9.

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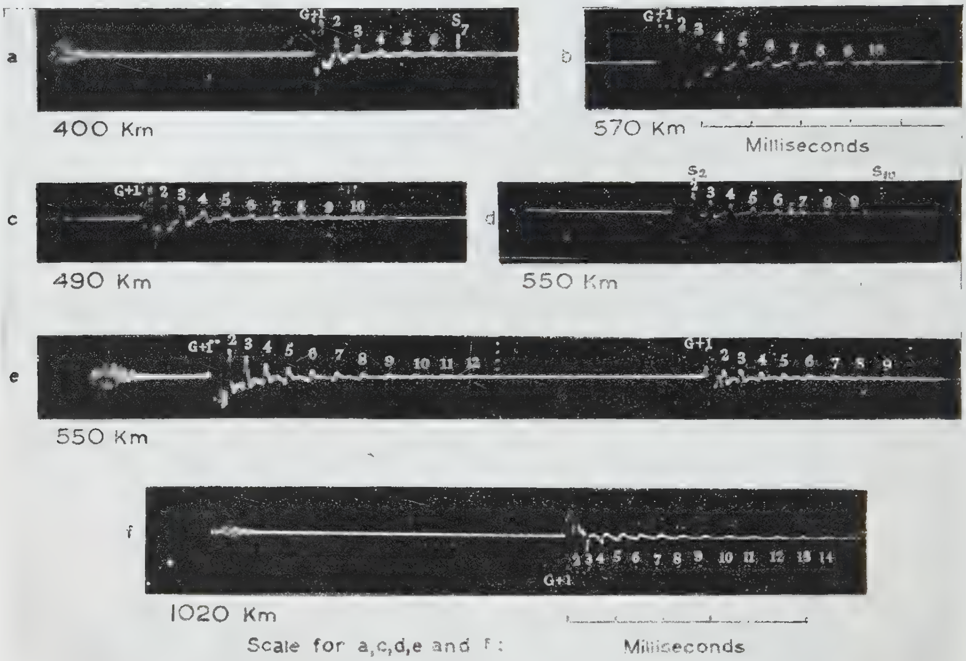

Figure 11.

(Reproduced by courtesy of the Council of the Royal Society.)

between them. The number of "waves" in the train is much larger in the night records.

The explanation of this complexity has been shown to lie in the fact that the radiation arriving at a distant point comes by many paths. After the direct or ground wave comes a wave which has undergone one reflection at the ionosphere, then waves which have undergone one or more multiple reflections between the ionosphere and the earth. The effect is clearest at night, since the conditions for reflection are then most suitable and attenuation is much reduced. As many as forty pulses, the last of which has experienced thirty-eight multiple reflections, are often observed.

If t_n is the interval of time between the arrival of the ground-wave pulse and the nth sky-wave pulse, we have

$$ct_n = \sqrt{4n^2h^2 + d^2(1 + h/r)} - d,$$

where c is the velocity of travel, h the height of the reflecting layer, d the great-circle distance to the original flash, and r the radius of the earth. This equation was first given by Laby and his associates in Australia, who suggested this interpretation of atmospheric wave-forms, though they did not succeed in demonstrating its complete applicability.

The application of this expression to the night wave-forms has yielded much interesting information. The values of t_n found by us agree very closely with those calculated with constant values of c and h, c being taken as the velocity of light. This indicates that both the velocity and the height at which reflection takes place are largely independent of the path travelled by the waves. An example is given in the table below, in which the mean observed values of t_n from five atmospherics from a storm are compared with values calculated for a distance of 204 km. and a layer-height of 88·7 km.

	t_1	t_2	t_3	t_4	t_5
Observed	229	683	1222	1782	2352 μsec.
Calculated	225	687	1221	1782	2355 ,,

Some examples of tracings of characteristic wave-forms at increasing distances are shown in figute 10, and some photographic records in figure 11. In this figure, records *g* and *h* are of the same flash in Johannesburg and Durban at different distances.

Independent check on the values of d has been obtained from cathode-ray direction finders operating at Johannesburg and Durban on the same atmospheric.

It is possible to derive the value of h, eliminating d, from a single good wave-form at night with an accuracy of one kilometre. The values so obtained are very consistent and yield a height close to 90 km. The results of a series of night observations are shown in figure 12.

The day observations obtained by us suggest a different type of sky-wave return—from a volume rather than a sharp boundary—with an effective height of 60 km. From the examination of night wave-forms it may be concluded that the general complexity of the atmospheric both by day and by night arises from effects produced in transit from the parent lightning flash; it does not orginate from the flash itself.

There is much interesting information to be gained by further studies of this kind. In the various component pulses we are able to examine the radiation field emitted from the original channel at various angles. These range from the direction parallel to the earth's surface (the ground wave) to that very nearly perpendicular to it (the sky wave of high order). No two atmospherics can give precisely the same amplitude distribution amongst the reflected pulses, since this distribution depends upon the angle between the sky rays and the channel of the discharge, which is never straight.

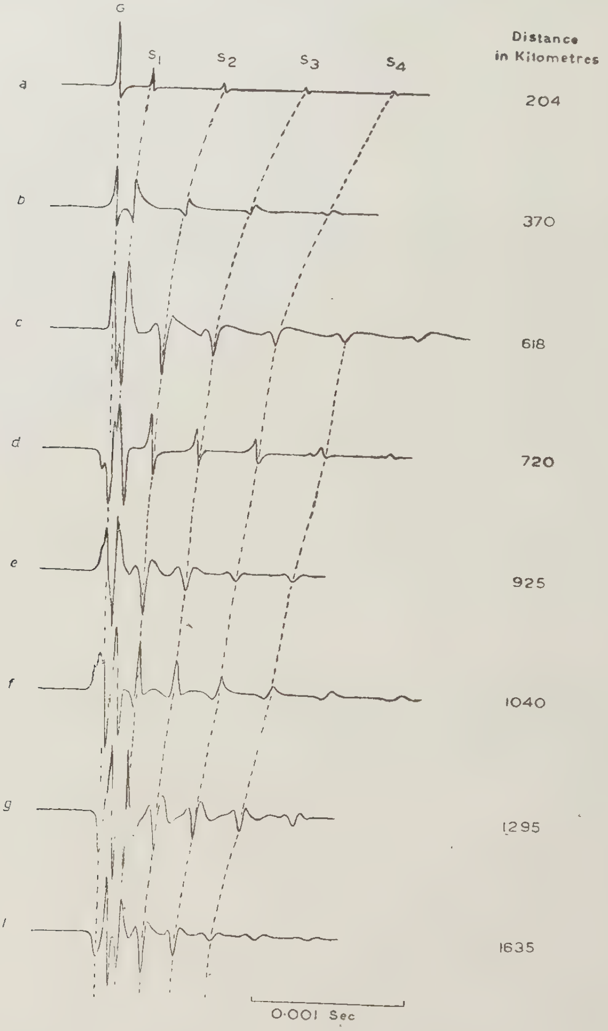

Figure 10. Atmospheric wave-forms at various distances.
(*Reproduced by courtesy of the Council of the Royal Society.*)

When we separate the ground pulse from the others, as we can do at night for not too distant storms, it shows the simple form which would be expected from the single surge of current in the return stroke, and, indeed, tells us a good deal about this surge and its form.

From such studies of the radiation fields of distant flashes we may obtain useful statistical information on the magnitude and duration of the current surges which produce them. Thus it has recently been shown by McCann and Morgan from an analysis of available records of the wave-shape of radiation fields that they are produced by strokes with currents whose times of decay to half value range from 20 to 100 μsec., an average of about 30 μsec. This is in agreement with the Boys camera figures for the return stroke and with direct measurements of lightning-stroke currents. More detailed records of the radiation field should yield information of value to the engineer on the wave-shape of the parent currents and in particular of their rising fronts, on which direct information is extremely difficult to get.

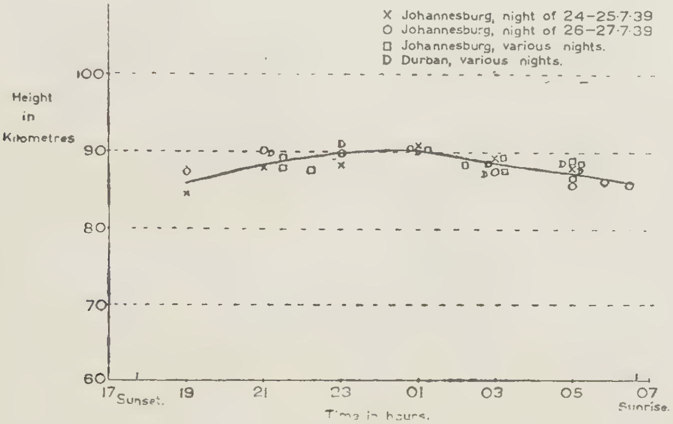

Figure 12. Height of reflecting layer at night deduced from wave-forms of atmospherics.

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To the physicist, however, the further study of the wave-form of atmospherics is chiefly interesting, because it offers a new tool for the study of the lower ionosphere. The method is likely to be of use, for example, in the study of changes in this region during total eclipses of the sun. Records of the wave-form of atmospherics have already been made by J. S. Elder, of the Bernard Price Institute, during the total eclipse of October 1940. These showed both day and night wave-forms and indicated that much might be learned from such observations.

§ 11. CONCLUSION

I have said nothing of the problem of the origin of the electricity in thunderclouds or of the quantitative aspect of these effects of lightning. Both these questions are suited for a more critical survey than the present one, for both are still too deficient in observational material for final presentation. There are other effects of the thundercloud, in particular its direct action on the ionosphere, which remain for further study.

But enough has been said to show that, like all geophysical problems, this one of the lightning discharge has amply repaid, by the value and interest of the results obtained, the labour and ingenuity expended on it.

There are many such problems in the field of geophysics, and it is to be hoped that they will receive the attention of the ablest of our younger physicists when they are once more free to work upon them.

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CHANCE, FREEWILL AND NECESSITY IN THE SCIENTIFIC CONCEPTION OF THE UNIVERSE

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The honour of being invited to deliver the Guthrie Lecture is one which I appreciate very highly, and for which I beg to express my most grateful thanks. The subject of the discourse having been left to my own choice, I propose to consider the association which has often been held to exist between the philosophical theory of determinism, on the one hand, and the scientific view of the world on the other: with especial reference to the question as to whether recent researches on the axiomatics of quantum theory have introduced new factors into the discussion on necessity, chance and freewill.

§ 1. INTRODUCTION

Physical science is concerned with the connexion between the past, the present and the future: it furnishes rules by which phenomena can be predicted from a knowledge of antecedent events. Remembering that no physical influence or effect can be transmitted at a speed greater than that of light, we may state the fundamental principle of classical physics thus: *It is possible to predict the physical conditions at any point P at any instant* t, *when we know the physical conditions, at any earlier instant* t_0, *at all points of space whose distance from P is not greater than* $\boldsymbol{c}(t-t_0)$, *where* $\boldsymbol{c}$ *is the velocity of light.*

This, of course, implies that strict determinism holds over the whole domain of classical physics. Now, when any general principle is discovered in one of the special sciences, the mind accustomed to its operation tends to imagine it typical of all Nature, to widen its field of application, and, in the absence of any evident limitation, to postulate its universal validity. In this case there seemed to be some justification for extending the law of necessity from material to non-material phenomena: for it was believed that psychical events were always correlated to physical changes; and if the physical changes were deterministic, the psychical events could hardly be otherwise. Thus support was found for a general philosophical theory which denied freewill and asserted that all volitions are governed by antecedent circumstances.

It may be remarked that the theory of universal determinism is not in itself natural or obvious, and, indeed, has never been arrived at except by trying to find a doctrine about volitions which will fit harmoniously into a general conception of the world, based on some other previously established principles. An illustration of this is furnished by the earliest school of thoroughgoing determinists, the ancient Stoics. In their philosophy, the universe was regarded as a closed material system, corresponding to a certain stage of the divine evolution. They had no knowledge of those laws of nature which enable the modern physicist to make predictions; but they did, nevertheless, make predictions, by practising the art of divination; and in order to make their doctrine compatible throughout with the belief that the future is predictable, they sacrificed freewill, and taught that all things in heaven and earth are governed by a principle of *fate* or *necessity*.

The Stoics were typical of the determinists of all ages, who have invariably been intellectuals, dominated by the ideas of consistence and unity, and consequently favouring a monistic type of philosophy. They have never had much following among ordinary men, whose reluctance to join them is easily understood: it rests upon the innate consciousness of the freedom of the will (which, indeed, is never denied even by those who explain it away), and the concomitant ideas of human responsibility, of conscience, of merit and demerit, which fit more naturally into a philosophy of pluralistic type. Other grave objections to determinism appear when the inferences that may be drawn from it are considered. Recent researches in cosmology have shown that the universe cannot have existed for an infinite time in the past, at any rate under the operation of the laws of nature as we know them: there must have been a Creation. Now if the universe, once created, is a closed system, whose evolution from moment to moment is governed exclusively by strict necessarian law, then Shakespeare and Newton and Hitler must all have been implicit in a full specification of the primaeval nebula; and a mathematician of sufficient power, present at the Creation, would have been able to predict the whole course of history from a knowledge of the original positions and velocities of the nebular particles. Stated in this form, the determinist creed seems hardly compatible with reason and common sense: one can scarcely believe seriously that the world acquired its entire stock-in-trade in a single consignment at the Creation, and that it has received nothing since. Yet if we were to accept, as I think we must, the conclusion that the universe is not a closed mechanical system, but is continually open to the accession of fresh elements, then we should be confronted by a world process which would not be reducible to classical theory. It now becomes a matter of the first importance to know what is the character of the new constituents of Nature—are they material or are they psychical?—and to investigate the manner in which they make their first appearance. Perhaps the best hope of beginning to solve this problem is to examine, in the light of modern experimental knowledge, the concepts of *chance* and *causality*.

§2. CHANCE AND CAUSALITY

First, let us take *chance*. When a coin is tossed in the air, we say that whether it comes down heads or tails is a matter of *chance*. What we mean by this is simply that we do not know on which side the coin will fall. We do not mean that there is any real indetermination in the occurrence, but merely that we cannot make a confident prediction because we do not know the precise velocities of translation and rotation which were communicated to the coin by the thumb of the operator, or the exact mass and figure of the coin, or the density and resistance of the air, or the height of the point of projection above the ground. Let us call these, and the other relevant data which are unknown to us, the *hidden parameters*. Then an imaginary person to whom the values of the hidden parameters were correctly known would be able, by aid of the laws of dynamics, to calculate mathematically all the circumstances of the flight and to determine whether the coin would fall heads or tails.

If I may be allowed to invent a word, I will call the tossing of a coin *crypto-deterministic*: by which is meant that the phenomenon is in reality deterministic, although we cannot foretell its outcome on account of our lack of information regarding certain hidden parameters, whose values must be known if prediction is to be possible. In crypto-determinism, the appearance of contingency is a fiction imposed on us by our limited means of knowledge.

In classical physics the notion of chance occurs frequently, and always in the crypto-deterministic sense, as, for instance, in the kinetic theory of gases, a science which presents certain other features of interest in this connexion. A gas may be considered from two different points of view, which we may call the molar and the corpuscular. The molar treatment takes account only of such qualities as pressure, density and temperature, which are characteristic of a finite amount of the gas: we may call these the *molar variables*. The corpuscular or kinetic theory, on the other hand, brings into reckoning the positions and velocities of the individual particles of which the gas is composed: we may call these the *corpuscular variables*. Now, so long as we remain within the molar circle of ideas, all the quantities that occur in the theory—that is, the molar variables—are measurable with our instruments, and the laws that govern their variation in time are perfectly definite. In the corpuscular theory, on the other hand, we are concerned with configurations and motions of particles, which are not in practice observable, and which cannot be completely specified in terms of molar variables. We have, therefore, to bring in the notion of probability; when we are given the values of the molar variables, there is a certain definite probability that the co-ordinates and components of velocity of a particular molecule will lie within specified limits. The motion of the molecule is actually governed by the laws of dynamics, so there is no infringement of determinism; the corpuscular theory is, in fact, of the kind we have called crypto-deterministic, the initial values of the co-ordinates and velocities of the molecules being the hidden parameters. We can thus place the corpuscular and the molar theories in contradistinction to each other under several categories: the former deals with individual particles, the latter with statistical aggregates; the former represents the gas as composed discontinuously of molecules, the latter smooths out the discontinuities and treats it as continuous; the former is a detailed specification of the motion, the latter is incomplete and is concerned only with averages; the former is crypto-deterministic, the latter is completely predictable.

§ 3. PROBABILITY IN QUANTUM THEORY

Let us now pass to the newer physics. As is well known, all atomic processes can be regarded from two alternative points of view—the corpuscular and the undulatory. An example of the double possibility of description is furnished by the emission of light from the excited molecules of a gas. In corpuscular language, there is a definite probability λ that, within the next second, any one excited molecule will fall to the ground state, emitting its absorbed energy more or less instantaneously as a photon, so that the number of excited molecules decreases proportionately to $e^{-\lambda t}$, and consequently the intensity of the emitted light decreases in the same proportion. In the undulatory picture, all the excited molecules are regarded as beginning at the same instant $t=0$ to emit wave-trains, whose amplitude diminishes exponentially, with the damping-constant λ, so that again the intensity of the total emitted light is proportional to $e^{-\lambda t}$. The two representations lead to the same formula, but there are differences between them which recall those between corpuscular and molar theories in classical physics: in the present case, the corpuscular description represents the phenomemon as discontinuous, while the undulatory description represents it as continuous; the latter is evidently deterministic in the fullest sense, but in the former, on the other hand, the individual emissions are not predictable, and the question therefore arises as to whether the corpuscular theory is crypto-deterministic, or whether a genuine indeterminacy is involved. To answer this it will be necessary to study some further examples of the double representation.

Let us consider, say, the diffraction of cathode rays. In the well-known experiment performed by Davisson and Germer in 1927, a beam of slow electrons was reflected at the face of a crystal of nickel; the crystal lattice acted as a three-dimensional grating, the electrons being reflected with special intensity in particular directions, which were the same as those obtained in the diffraction of x rays of a certain wave-length. Thus, although all the electrons originally had the same velocity and direction, some of them were reflected into one diffraction band and some into another; and while there was a statistical regularity, there was no possibility of predicting what fate would befall any particular electron. In this behaviour there is evidently an element of chance. On the other hand, the phenomenon of electron diffraction can be interpreted also from the undulatory standpoint, the cathode beam being regarded as an incident wave: the mathematical treatment is similar to that employed to explain the diffraction of ordinary light in classical optics, and there is complete predictability; that is to say, the directions in which the beam is diffracted are furnished by the calculations. In the particle representation, these directions appeared as statistical properties of the aggregate of electron paths, which in the wave-picture are not mentioned individually. It is evident, therefore, that the two theories bear to each other much the same relation as the molar and corpuscular theories of a gas in classical physics: one of them is predictable, the other is either crypto-deterministic or else has true indeterminism.

The close similarity between the wave interpretation of electron diffraction and the classical theory of the diffraction of light suggests that the latter phenomenon may have another aspect which is not completely predictable. A lecture-experiment devised by Professor Pohl of Göttingen is helpful in answering this question. Pohl, having produced interference effects by means of Fresnel mirrors or similar apparatus, observed the fringes with a photo-cell having a fine slit which was moved slowly across the interference pattern. The liberation of electrons was, by a suitable arrangement, caused to generate sound, so that a cracking noise was heard, which became greatly intensified when the slit was passing over the bright fringes. Thus the interference pattern, which seems to be purely deterministic when studied by the methods of wave optics, is shown to be fundamentally of a statistical nature.

Next, let us consider the phenomenon of radioactivity. As is well known, every atom of radium is liable to a spontaneous disintegration, in which an alpha particle is given off, while the atom is transmuted into an atom of radium emanation. The alpha particles emitted by a small quantity of radium salt may be observed by means of the scintillations they produce on a fluorescent screen, and these scintillations appear at irregular intervals. It is impossible to predict the instant when any particular radium atom will explode: it may be within the next minute or it may be centuries hence. All we can say is that the probability of disintegration within the next year is constant throughout the atom's existence, being, in fact, $4 \cdot 1 \times 10^{-4}$; whence we can calculate that the average life of a radium atom is 2440 years.

As was shown by Gamow and by Condon and Gurney in 1928, radioactivity may be treated by the methods of wave mechanics, the escape of alpha particles being represented by a gradual leaking-out of waves from the atomic nucleus. The mathematical equations furnish an expression, which is confirmed by experiment, for the decay constant of the radioactive transformation in terms of the velocity of the alpha particles emitted and the atomic number of the element. But the wave, which is continuous and completely predictable, takes no account of individual alpha particles, and offers no explanation of why one radium atom explodes sooner than another, and whether the phenomenon is crypto-deterministic or truly indeterminate. To see if the former explanation can be justified, let us recall that a radium atom is defined as an atom whose nucleus is formed of 88 protons and 138 neutrons. Is this a complete specification? In other words, are all radium atoms identical? If our test of the identity of two things is that they behave exactly alike when subjected to the same conditions, then clearly all radium atoms are not identical, since they have different lifetimes. Now in the definition of radium, nothing was said about the relative positions or velocities of the protons and neutrons inside the nucleus; and it might therefore be conjectured that these positions and velocities are the further data that are required in order to determine the instant at which the explosion will take place—that they are, in fact, "hidden parameters". On this view, the disintegration of a radium atom is crypto-deterministic; the notion of "chance", as it occurs in connection with it, is precisely the same as the notion of "chance" in connexion with the tossing of a coin; in both cases the system is in reality strictly deterministic, but our ignorance of the values of the hidden parameters inhibits us from making predictions.

Plausible though this interpretation may seem, there is good reason to believe that it is false. This will appear from the analysis of yet another experiment. Consider a beam of plane-polarized light which is passed through a Nicol prism, and let ϕ denote the angle between the planes of polarization of the incident and emergent beams, so that a fraction $\cos^2\phi$ of the intensity is transmitted, while the complementary fraction $\sin^2\phi$ is reflected. Let us interpret this experiment in the corpuscular way; then, whether a photon in the incident beam is transmitted or reflected is a matter of "chance", the probability of transmission being $\cos^2\phi$ and the probability of reflexion $\sin^2\phi$. What underlies the notion of "chance" as it occurs here? If the tossing of coins is a precedent, then we must postulate the existence of hidden parameters, which determine by strict necessity how the photon behaves. To simplify the discussion, suppose that there is only one hidden parameter, which can take one of two alternative values, say unity and minus unity, and that any photon for which the hidden parameter has the value unity is transmitted, while any one for which it has the value minus unity is reflected. Then, out of N photons, before they enter the Nicol, $N \cos^2 \phi$ must have the value unity, and $N \sin^2 \phi$ must have the value minus unity, for the hidden parameter. But when the photons have not yet become incident, they are unrelated to the angle ϕ, which specifies the arbitrary orientation of the Nicol. Thus the assumption which was made regarding the hidden parameters leads to an absurdity; and similarly any other attempt to explain the behaviour of the photons by postulating an underlying determinism can be shown to fail.

Thus the choice between the alternatives of transmission and reflection at the Nicol is truly indeterminate, so far as individual photons are concerned, and is qualified only by a statistical regularity when great numbers of events are considered. Once this very revolutionary principle has been accepted as valid for the experiment we have described, little difficulty is found in extending it to quantum phenomena in general, such as the disintegration of the radium atom.

By arguments such as the foregoing, many of the most profound writers on the axiomatics of quantum theory have become convinced that in atomic physics there is a genuine indetermination, a contradiction of the postulate of causality. So momentous a conclusion cannot be accepted without a close scrutiny of the process by which it is inferred; and there is, in fact, one element in the reasoning which is not altogether satisfactory, namely the tacit assumption that the photon has an individual existence, like that of an object in molar physics, which enables it to preserve a recognizable identity throughout the events in which it takes part. Now it is a well known principle of quantum mechanics that photons and electrons do not resemble ordinary particles as regards the quality of distinguishability. The histories of the single members of an aggregate cannot be followed: a photon or electron emerges only fitfully from a collective photonhood or electronhood. Thus, while on the one hand the behaviour of a photon passing through the Nicol prism can be predicted only statistically, there is on the other hand the compensating fact that it has, so to speak, only a statistical existence.

These considerations, however, while they weaken the argument previously given, do not amount to a rehabilitation of determinism; indeed, it may be felt that all they do is to make the whole matter more incomprehensible. In any case, they do not invalidate the more elaborate mathematical proof by which Professor von Neumann * has shown that "chance", as manifested in quantum phenomena, cannot be accounted for by postulating hidden parameters, but represents a true indeterminacy. The bonds of necessity have been broken; for certain classes of phenomena, crypto-determinism is definitely disproved.

To the modern man of science this conclusion is not only novel and

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* *Mathematische Grundlagen der Quanten-Mechanik* (Berlin, 1932).

unexpected, but almost incredible: and yet, like almost every other possible conception of Nature, it had been anticipated in the speculations of ancient philosophy. Indeed, the transition from Newtonian physics to quantum theory is in some respects parallel to the transition from Democritus to Epicurus in the history of Greek atomism. The older atomists, Leucippus and Democritus, were determinists; but Epicurus, while adopting their ideas as to the constitution of matter, thought it necessary to account in some way for the existence of human freewill; and this he did by endowing the atoms with a spontaneous power of fortuitous deviation in their motions, *exiguum clinamen principiorum*, as his expositor Lucretius says,* "a minute swerving of the ultimate elements". To this deflexion of the atoms he attributed the possibility of freewill in men, the faculty of initiating change *de novo*.

Incidentally the fortunes of Epicurus's speculation illustrate the dependence of the philosophy of any epoch on the state of scientific knowledge at the time. For the recent revolution in physics, which was brought about by experimental research, has suddenly compelled the acceptance of the philosophical doctrine that events may happen which are not the consequences of antecedent events—a doctrine which a generation ago was commonly regarded as beyond the bounds of possibility. It did, however, maintain a precarious existence in dictionaries of metaphysics and ethics, under the title of *accidentalism*; and in the last decade of the nineteenth century it was advocated by the American logician Charles S. Peirce, who gave it the name of *tychism*. His thesis was that chance of a kind that cannot be reduced to determinism by postulating hidden parameters, is an important factor in the evolution of the universe.

The doctrine of Epicurus connecting the spontaneous deviations of atoms with the freedom of the will is recalled by some recent utterances of Sir Arthur Eddington: "If the atom has indeterminacy", he says,† "surely the human mind will have an equal indeterminacy: for we can scarcely accept a theory which makes the mind to be more mechanistic than the atom." The inference is plausible, but I venture to think that it is not irresistible. For, although we have now accepted indeterminacy in microphysical processes, we continue to regard molar physics as predictable; and the two antagonistic conceptions are harmonized by the consideration that a system composed of a great number of random elements may, by the operation of the "law of great numbers", exhibit a regular behaviour; thus, for example, a gas consisting of molecules moving in a chaotic manner nevertheless obeys definite laws connecting temperature, density and pressure. Hence we could, if we wished, escape Eddington's conclusion by assuming that the human mind is a highly complex entity, whose observable behaviour is fundamentally of a statistical character.

However this may be, it is evident that a serious breach has been made in necessitarianism; and it is therefore desirable to approach the problem from a new direction, by inquiring more carefully into the association of determinism with the postulate of causality.

Science may be defined in general terms as the attempt to understand the world by tracing the connexion of events with each other; and this is effected by ordering our experiences according to the category of cause and effect, discovering for every phenomenon its determining agents or antecedents. The affirmation that this connexion is all-embracing, that no event happens without a cause, constitutes the *postulate of causality*, which, in the form adopted by Kant,* runs: "*All changes take place according to the law of the connexion of cause and effect.*" In Kant's view, the postulate is an *a priori* epistemological axiom, independent of all experience, and is a necessary presupposition of any knowledge that goes beyond the mere chronicling of events; so that if the postulate were invalid, the foundation of natural science would be destroyed. Undoubtedly we have to assume the existence of causes in order to arrive at the conviction that there is an external world which is the "cause" of our sensations; and this assumption provides the stimulus to all scientific research. But Kant's postulate goes beyond the mere assertion that causes exist, and affirms that there is no such thing as an uncaused event; moreover, in the application to physics, the word "cause" has been tacitly restricted to mean "an antecedent phenomenon of a purely physical character", so that the postulate has become tantamount to a declaration that physics is a closed order and perfectly deterministic. It seems impossible to accept this doctrine any longer, in face of the discoveries of modern quantum theory; and we must, therefore, subject the notions involved in the postulate to a careful analysis, in order to see what can be saved from the wreck.

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* *De Rerum Natura*, II, 292.

† *Math. Gaz.* **16** (1932), 66.

First, we must not forget that the relation of cause and effect is a matter not of direct perception, but of inference. In a typical case we observe in some phenomenon certain elements or phases which occur in succession whenever the phenomenon is repeated. This character of invariable sequence is the original fact of experience from which it may be possible to lead up to a judgement of cause and effect. But habitual antecedence does not in itself imply causality; for instance, the day is not the cause of the following night.

What, then, is a "cause"? The word belongs to common speech, and doubtless referred originally to the experience that we can by our own spontaneous activity bring about changes in the world: the changes are the "effect"; our activity in producing them is the "cause". As Mr. Bertrand Russell says: "The conception of 'cause'—however loath we may be to admit the fact—is derived from the conception of 'will'". This assertion is borne out by the history of scientific terminology; for in Aristotle's universe, the efficient "cause" of the regular movements of the celestial spheres was affirmed to be intelligence, operating in a way analogous to human volition. When Aristotelianism was superseded by the mathematico-mechanical Newtonian physics, the word "cause" was retained, now stripped of its anthropomorphic associations; but it was under some suspicion on account of its former connotation, and an attempt to explain it away was made by Hume, who argued that experience can never yield us anything more than the recognition of invariable sequence; that the origin of our idea of necessity is the contemplation of habitual uniformities, which generates in us a habit of expecting the second member of a sequence whenever its first member is observed; and that nothing more is contained in the "relation of cause and effect" than the assertions that the sequence in question has often occurred in the past, and that we can frame predictions in the confidence that the correlation thus recognized will continue in the future. The ground of our confidence must evidently be a belief in the order and uniformity of Nature; but this, in turn, may be explained as having been obtained by induction from the recollection of innumerable successful predictions in the past. Thus there is nothing in causality that is not derived inductively from repeated observation: the notion of cause and effect is essentially empirical, and is wholly different in character from, e.g., the sequence of premise and consequence in logic.

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* *Kritik der reinen Vernunft*, Elementarlehre, II Teil, 1 Abteilung, II Buch, II Hauptstück, 3 Abschnitt, Analogie B. The above is the statement of the Postulate in the second edition of the *Kritik*, which differs from the first edition at this point.

By accepting such a doctrine we should certainly escape many difficulties: causality becomes a more flexible concept when it is based on the Principle of Induction rather than on Kant's Postulate, and it can be used freely without involving us in the false assumption of universal determinism; for if the causal law is derived from experience, exceptions to it may be revealed by further experience. In tracing its origin back to the observation of sequences, however, we must not confuse it with them. Many habitual sequences are not causal, and, indeed, the task of the scientific worker is, in a great measure, to distinguish causal from non-causal correlations, and to avoid the fallacy of *post hoc ergo propter hoc*. As scientific knowledge becomes more developed and precise, relations of causality tend, in physics at any rate, to become expressed by mathematical laws, generally in the form of differential equations, from which the sequence of events can be deduced by a process of integration. The word "cause" itself does not appear in the statement of the differential equations, and the Principle of Causality is reduced to the simple assertion that all phenomena are completely determined by antecedent phenomena. This may be called the *physical concept of causality.*

In contradistinction to this stands what may be called the *psychological* concept of causality, which is derived from the notion of will. The upholders of the physical concept try to dissociate the notion of cause from the notion of will, and to reduce it to something non-vital and abstract, an attitude which was common enough during the heyday of Newtonian physics. But man is, after all, a part of the Universe, and a very important part. It is in the light of his experience that Nature must be interpreted; and the attempt to leave him out and to express knowledge in a completely impersonal and dehumanized form, while doubtless practicable in the case of special sciences such as physics and chemistry, is a radical error from the point of view of the philosopher who is trying to apprehend reality. The problem of causality is, broadly speaking, that of the derivative origin of phenomena; and the study of it cannot be restricted to cases where the originating and the derived phenomena are both of a purely material character. It is necessary also to investigate the more interesting, even if more difficult, cases where mind and matter interact. Account must be taken of events which have a character of uniqueness, such as those of personal history, which are not amenable to treatment by mathematics. It is evident, therefore, that differential equations are inadequate to cover the whole field, and that the psychological is the primary and more general concept of causality. The importance of the question for philosophy depends precisely on the circumstances that it extends across the gap which divides the psychical from the physical, and involves the fundamental problem of human freewill.

However, in the moral, as in the material, realm, it is possible to conceive of a strict determinism, according to which all that happens at any instant is, in principle, predictable from a knowledge of what was happening at any given previous instant. In the words of a distinguished modern advocate of this position: " Determinists maintain that our volitions are as completely determined as all other events. From this it is generally, and I think correctly, held to follow that it would be ideally possible to deduce the whole of the future course of events from the present state of reality—though, of course, a mind enormously more powerful than ours would be required to do it".*

The doctrine which stands in opposition to this moral necessitarianism is somewhat different in character from the anti-determinism of physics. As we have seen, in the new conception of the material world which has resulted from quantum theory, there is in certain classes of events an unpredictability which is a true indeterminism, i.e., which cannot be reduced to determinism by postulating the existence of hidden parameters: the universe is, in fact, represented as open at every instant to influences not derived from its antecedent state. In the moral sphere also the crucial question is whether there can be an intrusion into existence of things that are new, in the sense of being not deducible from a complete knowledge of antecedents. But here the problem is changed, inasmuch as we have now to deal with a living *determining agent*. The moral necessitarian has the onus of proving that the human personality together with the external world constitutes a closed system in which nothing ever originates, in which whatever takes place is only the mathematical consequence of what existed before. It has already been shown that the material world, apart from personality, is not a closed system; and one can scarcely expect that the adjunction of personality will make it one. Indeed, in the realm of personality, what (at any rate, at first sight) appear to be new intrusions are familiar to everyone.

For instance, two drunkards stagger together into a Salvation Army meeting and hear an address. One of them is reformed on the spot, becomes a teetotaller, and ultimately rises to be Mayor of his native town: the other is unchanged. If this undoubtedly possible phenomenon is to be reconciled with determinism, it becomes necessary to have recourse to the hypothesis of hidden parameters, situated, presumably, in the subconscious mind, and differing for the two men. But there is no independent evidence for the existence of these hidden parameters, and apart from the motive of explaining everything deterministically, there is no reason for believing in them. The determinist here is not proving determinism, but assuming it, and merely indicating how the phenomenon would have to be accounted for, if everything had to be interpreted in terms of the necessitarian scheme of thought. The man's own description of his experience is that he has entered on a new life, that he has been born again; and since we now accept the principle that intrusions—events which do not pre-exist in antecedents—can make their appearance in the physical world, we are not indisposed to accept the same principle in the mental domain, and to contemplate at any rate the possibility of uncaused creative moments in the life of the psyche.

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* McTaggart, *Some dogmas of religion* (London, 1906), p. 145.

With this prepossession, let us consider more closely what is involved in willing. We may distinguish two different kinds of conscious internal inclination. There is, first, animal desire, the *appetitus sensitivus* of the scholastics, or, in a more recent terminology, that which wells up into the Ego from the depths of the Id. This is generally directed towards some concrete object which promises pleasure, and is characteristic both of animals and of men. Secondly, there is the inclination which springs from attachment to some ideal—some universal, conceived by the mind. This, the *appetitus rationalis* of the scholastics, desires concrete objects only in so far as they promise to lead to the fulfilment of the ideal: it is characteristic of the one self-conscious and reflective creature, the only possessor of a super-ego, namely man. In it there is involved first the craving for an abstract good, then the presentation to our minds of some particular good that might be realized, then reasoned deliberation as to how it shall be brought about, and then (in a typical case) a struggle within the personality between the purpose thus conceived and the animal appetites, a struggle which may issue one way or the other. The *freedom of the will* may be *defined* as the power of acting according to abstract conceptions or ideals, in spite of animal desires. It is thus simply a name attached to a certain phenomenon—a phenomenon which undoubtedly often happens—so that, with this meaning of the words, there is no question as to whether the freedom of the will exists or not. The question is whether it ought, or ought not, to be interpreted deterministically.

Let it be observed first that the freedom of the will, thus understood, is not something innate, but something to be won: in the hierarchy of values it has the high place that is associated with victory in moral conflict. It is not (what it is sometimes misrepresented as being) unmotived volition, mere indeterminacy; this, indeed, would be morally worthless.

The necessitarian, who asserts that moral decisions are governed by physical determinism, employs a terminology which greatly assists his case, by suggesting that the factors involved in an act of the will resemble those which are familiar in the consideration of mechanical systems. He speaks of "motives" without distinguishing different types of motive, so that for him the only significant thing about a motive is its strength; and thus he is enabled to regard motives as broadly analogous to forces in dynamics. When he comes to consider the part played by the living agent, he minimizes or suppresses the most important characteristics of personality—its capacity of being an object to itself, its spontaneous endeavour and creative activity, its integration of body, soul and mind into a complex unity, and its abiding continuity as a causal self behind the phenomena of its history; the notion of personality is, in fact, reduced to that of "character", conceived as an objective entity, a kind of precipitate left behind by past experiences which could themselves be accounted for deterministically; its reaction to any situation is assumed to be determined by its nature, which at any particular moment is quite definite. The character, automatically and deterministically, fixes the strength of the various motives which the mind presents to it as suggestions; and the motives, thus quantified, engage in a tug-of-war, in which the strongest wins. The living agent, apart from his possession of the "character", is no more than the point of application of the forces, which lies in helpless passivity and is constrained to move in whatever direction it may be impelled by their resultant. This account ignores the fact that the two types of inclination, the *appetitus sensitivus* and the *appetitus rationalis*, are linked to the personality in entirely different ways, and that it is precisely this difference of linkage which is the source of the freedom of the will. The man who is attracted to an ideal can, indeed, contemplate with loathing himself, exposed at the very same moment to the tension of animal desires; and this drama of the soul, this duplication of the personality into the *objective*, which contains the phenomena under consideration, and the *subjective*, which apprehends and deals with them, bears no resemblance whatever to mechanical forces acting at a point. The will is the man determining himself, and in all willing he is his own object.

It is, of course, essential to the necessitarian case that the "character" should have been generated deterministically in the past. Thus, the inquiry cannot be restricted to a consideration of what happens at the instant when the decision is made, but must extend over all the agent's previous life. The determinist has to prove that the personality, the man himself, as he exists at any moment, could have been completely specified in terms of what he was at some previous epoch, together with the state of the external world at that epoch. I am not acquainted with any serious attempt to establish this position; and the analogy of the inanimate world, as we have seen, goes against it. We are thus, I think, justified in holding that the interior life is not crypto-deterministic, that the personality plus the external world is not a closed system, that in the psychical, as in the physical domain, there is a continual succession of intrusions or new creations. The universe is very far from being a mere mathematical consequence of the disposition of the particles at the Creation, and is a much more interesting and eventful place than any determinist imagines.

Over the field within which rational science is possible, there must be regularity, for on this we depend for a connexion between the past, the present and the future: but the regularity is not universal and exhaustive, for the world is being enriched unceasingly by the accession of new elements. It has been surmised that some relation, unknown to us as yet, may exist between the intrusions and the regularities. Peirce believed that the laws of Nature have resulted from an evolutionary process, the fixing of inveterate habits, so to speak, in the world of inanimate matter. But however this may be, the way in which the two features of regularity and novelty are ensured simultaneously, and in both the physical and the psychical spheres, compels our wonder and exaltation.

May I be allowed, in conclusion, to advert to a somewhat broader issue? In the nineteenth century the view was widely held that determinism was in process of being established beyond doubt by the advance of science, but was to be dreaded, because if accepted it would subvert the foundations of ethics and religion. It may be doubted whether these fears were well grounded; for the arguments by which Jonathan Edwards and the other philosopherdivines of the Calvinist succession attempted to prove that determinism is not necessarily antagonistic to morality and spirituality, are not without force. Indeed, it might be said that any theologian who believes in God's complete fore-knowledge can scarcely avoid some necessitarian affinities in his system, as witness St. Augustine. At any rate, since quantum physics has modified our ideas of the part played by determinism in Nature, the fears to which I have alluded should be dispelled. Nay, more, when it is remembered that new intrusions into the universe are placed by the man of science in the category of pure indeterminacy, but are regarded from the theistic point of view as acts of the Divine Will, and, therefore, part of a deterministic plan, it would seem doubtful whether the degree of approximation towards a necessitarian outlook is to any appreciable extent closer in science than in religion.

AN ELLIPTOGRAPH:

with Supplement on

THE EVOLUTE OF THE ELLIPSE AND THE ELASTICA

By Sir CHARLES V. BOYS, LL.D., F.R.S.

MS. received 20 *June* 1943

§ 1. INTRODUCTION

Draughtsmen so often disfigure their drawings by representing ellipses as a combination of four arcs of two circles that I have thought it worth while to produce an instrument which will, at one sweep, describe a half-ellipse which will pass exactly through the points marked on the paper for the ends of the axes, and not merely some ellipse. This works so perfectly that I am submitting a description of it to the Physical Society. The description of the instrument is simple enough, and it is greatly aided by the projection view (figure 1) which I have drawn by the method which I described in a paper on "Square-ruled Paper Projection" in the *Journal of Scientific Instruments* for May 1942. This is made directly from the dimensions of the instrument, without intermediate plans and elevations. It is, I think, a more convincing example of the value of the method than are the two dodecahedrons which I used in that paper. The discussion of the comparison between two wholly incompatible curves, the evolute of the ellipse and the elastica—or, I should say, a particular elastica—whereby they are shown to be almost indistinguishable one from the other, is more difficult and, to me, far more interesting, and I have thought it best to treat this separately in a Supplement.*

* Before proceeding, however, with either of these I should like, if I may be permitted, to make a personal statement. I was given a life membership of the Physical Society by the late Professor Guthrie, its founder, in the year 1880, and I was demonstrator of the Society for about 10 years. Of the many papers which I have contributed to the Society in the past sixty-three years one characteristic has been very common—an intimate combination of geometry and mechanics—and this will be found more especially in the Supplement of the present paper. In constructing the instrument and in making the drawings I have had to contend with the difficulty of having only one available eye, and that operated on for cataract six years ago. The absence of binocular vision and the inability to judge distance has been a great handicap, and I hope that imperfections in the drawing, mainly due to this, will be excused.

Any book on the differential calculus gives the equation

$$(ax)^{2/3}+(by)^{2/3}=(a^2-b^2)^{2/3}$$

as the evolute of the ellipse

$$x^2/a^2+y^2/b^2=1,$$

and states that if a pencil is guided by a "string" wound round a material evolute and of such a length as to make it pass over the ends of the axes, that pencil will describe the ellipse. I think it likely that many may have hoped that an elastic strip bent so that its ends are coincident both in position and direction with those of the evolute would serve as a material evolute, but if an elastic strip is merely bent the length of strip included between the fixed points will be so much more than the true length of the evolute, which is

$$a^2/b-b^2/a,$$

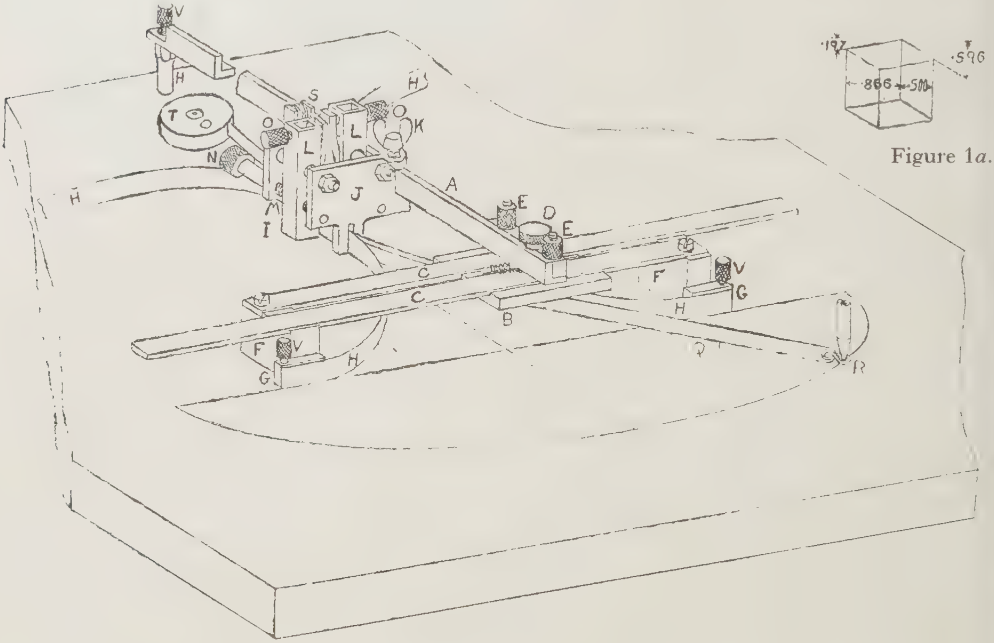


Figure 1.

Figure 1*a*.

that a pencil which passes through the end of the minor axis will fall short of the points which mark the ends of the major axis. It is true that we might calculate the form of the evolute and provide a material evolute of that form, but this is an impracticable proposition, and the problem remains unsolved. I have produced an instrument which enables the user to make such a strip assume the form of the evolute, not absolutely, as that is impossible, but so closely that either no difference can be detected, or if, with increasing eccentricity, it can be, as is the case where a is double b or more, even so the error in the ellipse drawn is very small. This is done automatically by the instrument, equally when the user has no drawing of the evolute with which to compare it. As, in any actual test, specific values for a and b must be taken, I have chosen

6 inches for a and 4 inches for b. Setting the instrument with these values, and using it to draw the ellipse and to trace its evolute against the strip, a series of calculated points at frequent intervals all lie so closely on the two curves that no error is noticeable.

What is needed is a longitudinal tension applied to the elastic band to make it less convex, and the correct amount is determined by seeing that the pencil guided through the end of the minor axis also passes through the ends of the major axis, and regulating the tension in the two bands separately by micrometer screws until this is the case. When this is done, no error as great as one-hundredth of an inch due to error in the evolute is produced in the ellipse, 12 inches by 8 inches, so drawn.

§ 2. DESCRIPTION OF THE INSTRUMENT

After this preliminary statement the construction of the elliptograph is made so clear by figure 1 and the detail figures which follow that description of the instrument is almost unnecessary, and so is very short.

Figure 1 is a projection view of the instrument resting on the paper on the drawing board, on which it has drawn a half-ellipse. Parts of the drawing board are cut away.

Figure 1 *a* is the key figure 1 (*d*) taken from the paper already mentioned. From this it will be seen that the view is taken from a direction in a plane inclined at 30° to the length of the instrument and looking down at an angle of 20°.

Figure 2 is the front plate of the carriage.

Figure 3 is the back plate of the carriage with spring swivel clip.

Figure 4 is a vertical section of one of the vices.

Figure 5 is a horizontal section under the carriage $\frac{1}{8}$ inch above the paper.

Figure 6 is a spare block holding an elastic strip and extended as described at the end of this paper.

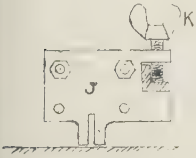


Figure 2.

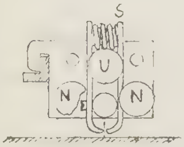


Figure 3.

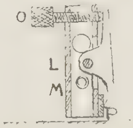


Figure 4.

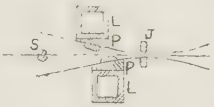


Figure 5.

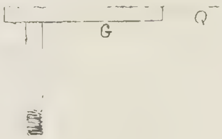


Figure 6.

The backbone A at the near end is carried over a plate, B, and between them are two racks, CC, engaged by a pinion, not visible in the drawing, which can be turned by the milled head D so as to move the two racks equally in opposite directions. The locking nuts EE clamp the two racks in position when desired. The racks each carry at one end a block, F, with a removable part, G, to which is secured, by sweating, the elastic strip H of spring-tempered steel, which is naturally straight. These elastic strips may be interchanged with others of a different degree of stiffness very quickly. I had the greatest difficulty in getting straight strips of spring-tempered steel. Hard-rolled mild steel takes a set and, therefore, is unsuitable. Finally, Mr. Ronca, of the Board of Trade, and Mr. Hope-Jones came to the rescue and provided me with three strips, 7/16 inch wide and 0·015 inch thick and ¼ inch wide and 0·010 inch thick. The pieces G form two of the three feet on which the instrument rests. The third is seen at H at the other end of the backbone. Somewhere on the backbone, which is **L**-shaped in cross-section, is the carriage I, in which the fine adjustment means are concentrated. The front plate J (figure 2) is provided with a saw-cut in a projection which reaches nearly to the paper and through which the two elastic strips pass. It also carries the clamping screw K by which the carriage may be secured in position. The two elastic strips HH pass through the jaws of two vices, LL, the construction of which is made clear in the detail figure 4, and out at the back, where they are bent. so as to be out of the way, by resting against weights which are outside the figure. The two vices LL can be made to travel a short distance on rods, which form part of the carriage, by means of two screws, MM, with milled heads, NN, for the purpose of independent fine adjustment. The vices can be made to pinch the strips by the action of the screws OO. The lower ends of the vices are provided with inclined blocks, PP, seen in the horizontal section (figure 5), between which and the lower ends of the levers the strips are pinched so as to get a firm hold. Between the two elastic strips a thin band, Q—actually a small Chesterman steel tape—of steel ¼ inch by 0·004 inch thick, is placed. At its free end it is provided with a pen or pencil guide, R, clear of the paper, while at the other end, after passing between the jaws of a fine adjustment swivel spring clip, S, it is seen wound up in its spring case, T. The construction of the swivel spring clip is made clear in figure 3. It is carried on the back plate of the carriage, and it is provided with a fine adjusting screw with a milled head, U. The spring clip may be made to let go of the tape by pinching the upper end and so compressing the pinching spring.

In order to set the instrument, the paper must have ruled on it the axes of the intended ellipse, and the points on these, which are the centres of minimum and maximum curvature of the ellipse. These are at points distant from the ends of the axes a^2/b and b^2/a, and the evolute quadrants meet in cusps at these points.

The instrument is set down on the paper, the locking screw K of the carriage is freed and the pinching screws of the two vices are also freed. Then, after loosening the locking nuts EE, the milled head D is turned so as to bring the outer ends of the evolute strips HH exactly over the two centres of curvature on the major axis. The nuts EE are then locked. Then the carriage is slid along the backbone until the saw-cut in the front plate of the carriage is exactly over the centre of curvature on the minor axis or minor axis produced. The two strips being quite free to take their natural position, are nearer the minor axis than is the evolute. This may be made evident by releasing the spring clip which holds the Chesterman tape, and with a pencil passing through the guiding loop R pulling it out until the pencil is at the end of the minor axis. Then, with the tape held in the clip, carry the end round to the ends of the major axis. It will be found to be short of these by 0·47 inch if the semi-axes of the ellipse are 6 inches and 4 inches. The elastica strips between the now-fixed end points may be shortened by withdrawing the vices by means of their travelling screws, but much time may be saved by marking on the paper one point on each side of the minor axis through which the evolute passes. If the distance between the centre of the ellipse and the saw-cut in the front plate of the carriage is bisected, and one-ninth of the distance between the other ends of the elastic strips is set out on each side from this point and a mark made, this mark will be almost exactly on the evolute. It is better, therefore, instead of making a number of trial-and-error settings by the aid of the travelling screws, to free each strip in the vice and pull the end by hand until the strip lies over the marked point. In the present case, the marked point will be 0·74 inch on each side of the minor axis. The evolute is 0·75 inch on each side. The travelling screw may then be used merely as a final adjustment to bring the pencil exactly through each end of the major axis. With the instrument thus set any number of half-ellipses may be drawn with almost the same facility that a circle may be drawn with a pair of compasses, and with almost the same accuracy, either with a pencil or with a pen.

It will be seen that each strip between the fixed points on the axes has now the same length as the evolute, and at each end it coincides with the evolute both in position and direction. With these five features identical, the strip must assume a form which is very close to the true evolute. In the Supplement I have discussed the relationship between the two curves.

There is one detail not yet mentioned. I have found that with the instrument made exactly as described there is some risk of its shifting on the paper when the ellipse is being drawn. To prevent this, I have provided each of the three feet with a fine screw with a milled head, V, terminating in a fine needle-point. When the instrument is being moved on the paper these screws are drawn back so that the points do not project, leaving them like the soft paddy paws of a friendly cat. But they are ready for instant use when required, and a turn or two of the milled heads causes them to penetrate the paper, so that accidental slipping is prevented. The holes made in the paper are no larger than those made by needle-point compasses.

The range of the elliptograph is somewhat limited, more especially in the eccentricity of the ellipse which can be drawn with it. When this exceeds 2 : 1 for a/b it begins to be awkward, and at 3 : 1 it is becoming impracticable as well as slightly inaccurate. If made as shown in figure 1 it is also limited to ellipses in which the ratio a/b is not too close to 1, for such ellipses of moderate size have evolutes too small to occupy rectangles of about 2 × 2 inches. But this limitation is not essential. If the blocks which hold the elastic strips are made as shown in figure 6, one side of the rectangle occupied by the evolute may be made as small as may be desired, and if the part of the front plate J of the carriage in which the saw-cut is made is carried on a bracket projecting under the plate B, the other side can be similarly reduced.

Where, owing to great eccentricity, the evolute becomes unduly extended, and there is a consequent risk of its being bent under the tension of the tape Q when the ellipse is being drawn, this may be prevented by placing a flat plate over the elastic strips and under the plate B, for which space is provided, and putting a weight on the plate so as to anchor the strips down on the paper.

SUPPLEMENT

§ 1. THE EVOLUTE OF THE ELLIPSE AND THE ELASTICA

The evolute of the ellipse and the elastica are curves which, mathematically, are wholly incompatible, yet as produced in the elliptograph described above the elastica assumes the form of the evolute with such accuracy that for ellipses no more eccentric than those in which a/b is equal to $3/2$ it is barely, if at all, possible to distinguish between them.

§ 2. THE EVOLUTE OF THE ELLIPSE

The evolute of the ellipse

$$(ax)^{2/3}+(by)^{2/3}=(a^2-b^2)^{2/3}$$

is a curve which it is tiresome to calculate, but this is best done with the aid of Barlow's tables, in particular the new edition, 1930. It is, however, quite unnecessary to calculate afresh every evolute which may be met with. Evolutes of all ellipses are projections of the evolute of any other ellipse, stretched or compressed differently in the two main directions. I prefer to consider them all as being projections of the same parent evolute, which I call the evolute of an infinite circle. The parent evolute is the curve which is the envelope of the centre line of a trammel when moving so as to describe an ellipse, and it is a standard example of an envelope in books on the differential calculus. But it is not so called, and I have never seen any suggestion that it may be looked upon as the evolute of an infinite circle. The equation given is

$$x^{2/3}+y^{2/3}=c^{2/3}.$$

This curve is obviously symmetrical in x and y, so that only one-half need be calculated to find it all, and the calculation of each point is much shorter than that of the evolute of the ellipse. If square-ruled paper is used on which to draw the curve it is convenient to make $c=10$ inches. Then the maximum values of x and y are each 10 inches and at the middle point $x=y=5\sqrt{2}$ inches. The description "evolute of an infinite circle" may seem to be a contradiction in terms, but it is not so, as any finite quantity is as 0 compared with the two equal infinite axes. Now to apply this to the case illustrated in this paper, an ellipse in which $a=6$ inches and $b=4$ inches. The evolute occupies a rectangular frame 5 inches × $3\frac{1}{3}$ inches, whereas the square frame occupied by the parent evolute is 10 inches × 10 inches. The frame and the curve within it are then obtained from the parent by dividing all ordinates by 2 and all abscissae by 3. Figure 7 shows the two curves in their frames with the projection of the middle point indicated. Generally the evolute of an ellipse occupies a frame which is

$$\frac{a^2-b^2}{b}\times\frac{a^2-b^2}{a},$$

and these are the quantities by which 10 must be divided to find the points on the evolute from corresponding points on the parent evolute. Further, dy/dx is equal to $-(y/x)^{1/3}$, and this, multiplied by a/b (or by $3/2$ in the example), is the dy/dx at the corresponding point in the evolute of the ellipse. Also dx/dy gives the direction of the radius of curvature. But what of its length?

The calculation of ρ, the radius of curvature, is much more tiresome, and I have not been able to find any way of deducing ρ for points in the evolute of an ellipse from the ρ of the corresponding point in the parent evolute.

But this much is apparent. If we start with a parent evolute in a frame of the same area as that of the evolute of the ellipse, i.e. of one in a square frame of which the sides are

$$\frac{a^2-b^2}{\sqrt{ab}},$$

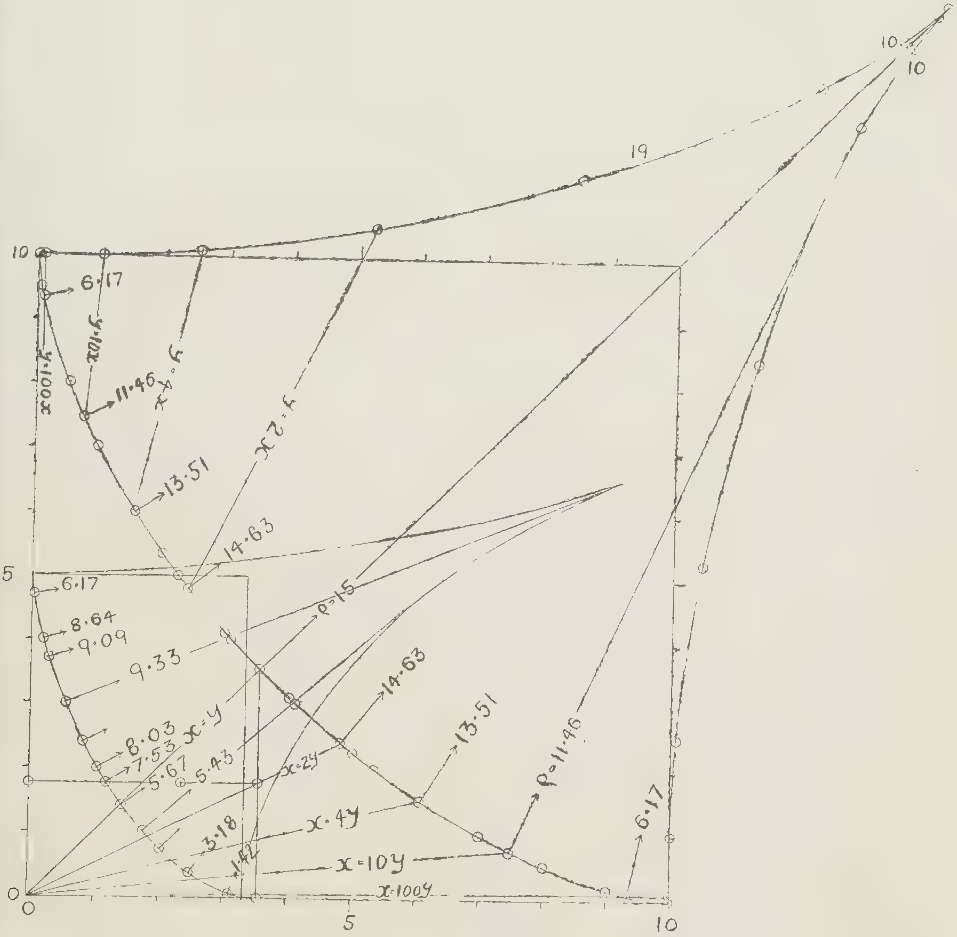


Figure 7.

so as to make the two curves comparable, those portions above the middle of the curve will, being compressed laterally and stretched vertically, become less curved, while conversely those below will become more curved. For this reason the position of least curvature moves up the curve from the point corresponding to the middle point of the parent evolute. The result is that the radius of curvature is a maximum near the point where $y=3{\cdot}5$ or where

$$\frac{dy}{dx}=\frac{1{\cdot}9}{5}=\tan 21^\circ.$$

I have found the following expression for the radius of curvature of any point in the evolute of an ellipse:

$$\rho = 3\,\frac{\{b^2x^{2/3}+a^2y^{2/3}\}^{3/2}}{a^2bx^{1/3}y^{-1/3}+\alpha b^2x^{-1/3}y^{1/3}},$$

and the same expression without any a's or b's gives the radius of curvature of the parent evolute at any point (x, y).

As I wanted to know the curvature $(1/\rho)$ at a number of points on the evolute of an ellipse, I hoped that I could obtain a short cut to this if I had the equation of its evolute. This I could not find, but my desire was conveyed to Professor E. H. Neville, professor of mathematics at Reading University. He very kindly sent me a luminous treatment of the general problem of what might have been called successive evolution if the word evolution had not already had a different meaning attached to it. However, I have not found that it has provided the short cut for which I sought, and so I have had to go through the labour of calculating the curvature at a number of points. I am not saying more about Professor Nevill's thesis, as he might prefer to develop it in a more purely mathematical medium. Another mathematician found a pair of equations to represent the evolute of the evolute of the ellipse, but again I did not find that these provided the short cut for which I had looked. By means of these two equations the values of x and y are derived from a series of values of a roving parameter θ.

There is a curious feature about the evolute of the ellipse, or of the parent evolute. The radius of curvature at the extremities is obviously equal to 0, because if either x or y is made equal to 0, one or other term of the denominator becomes infinite. But there is no sign of this in any drawing which can be made from the equation. The fact is that the whole extent of that part of the curve which comprises excessively small values for ρ is itself infinitesimal; it constitutes a kind of mathematical fiction, and it is completely ignored by the elastica, which otherwise almost exactly coincides with the evolute.

It may be worth while to point out that the calculation of the radius of curvature at any point of the parent evolute, as well as of the curve itself, may be greatly simplified by making $y=nx$ or x/n. Then, if this is to occupy a square of 10 inches, the radius of curvature becomes

$$\rho=\frac{30}{n^{1/3}+n^{-1/3}},$$

and with the aid of Barlow's tables it can be written down almost at once. The radius at the middle point is 15 inches and is there a maximum. The decrease is not very rapid, for even when $y=10x$ or $x/10$ it is still 11·96 inches. When $y=100x$ or $x/100$ it is 6·18, and even when $y=1{,}000{,}000$ or $x/1{,}000{,}000$ it is still as much as 0·3.

The rectangle containing the evolute of the ellipse ($a=6$, $b=4$) and the square containing the parent evolute are shown togther in figure 7. The numbers give the radii of curvature in inches calculated for the marked points. Also the evolute of each is shown.

It seems to me to be a very curious fact that the parent evolute is identical with a curve of quite a different class. This is a particular hypocycloid, that in which the rolling circle is one-quarter of the diameter of the one in which it rolls. A point on the circumference of a 5-inch circle rolling inside a 20-inch circle describes the same curve identically as the parent evolute in a 10-inch frame. This is convenient because, while all other evolutes of the ellipse are rectifiable by a simple subtraction, that of an infinite circle is not. But the hypocycloid is rectifiable, and each loop of the one described has a length of 15 inches (Todhunter, *Integral Calculus*, p. 80). This is the same as the radius of curvature at its middle point.

Finally, I have found that Joseph Edwards in his book *The Differential Calculus* has given an equation for the evolute of what I have called the parent evolute. It is

$$(x+y)^{2/3}+(x-y)^{2/3}=2a^{2/3}.$$

This is best calculated by writing $y=nx$ or $y=x/n$, and this I believe to be the only way, and the evolute may thus be plotted. It is shown in figure 7 outside the large square. But here again, though the calculation is simple enough, I have not found that short cut, for the expression which I gave,

$$\rho=\frac{30}{n^{1/3}+n^{-1/3}}.$$

can be calculated more quickly still.

§ 3. THE ELASTICA

On page 87 of his book on *The Elliptic Functions* Sir George Greenhill quoted from Thomson and Tait, *Natural Philosophy*, § 611, as follows:—"The elastica is the name given to the curve assumed by an elastic [strip] originally straight when bent into a plane by a stress composed of two equal opposite forces T, on the assumption that at a point P at a distance y from the line of the applied stress the bending moment Ty is equilibrated by a moment of resistance B/ρ proportional to the curvature $1/\rho$; and the constant B is called the flexural rigidity of the spring".

There are two kinds of elastica, the undulating elastica concave to the line of stress, which is compressive, and the nodal elastica convex to this line, which is a line of tension. As the nodal elastica is the only one with which we are concerned, I shall consider this only. It so happens that this was the curve which I chose to illustrate the use of an instrument for drawing curves by their curvature which I described in the *Proceedings of the Physical Society* for 1893. This made it possible to carry out the method described by Lord Kelvin and practised by Professor Perry, but with far greater speed and accuracy.

Greenhill gave the formula

$$dx/ds=\cos\theta=\sqrt{(1-4\,\mathrm{sn}^2 s/c\,\mathrm{cn}^2 s/c)}=1-2\,\mathrm{sn}^2 s/c,$$

and added "so that x is given in terms of s by means of elliptic integrals of the second kind". As elliptic functions are outside the range of my self-taught mathematics, I have determined by experiment the direction and position of the line of tension. But I thought it best to begin with the parent evolute, because in this case there could be no doubt as to the direction of the line of tension. From considerations of symmetry it must be at 45° with the sides of the frame. For elastic strip I took a hack-saw blade, AB, 3/8 × 0·024-inch, let down to a purple-blue spring temper. In its dead-hard condition it would snap short with very little bending. This was not long enough for the whole curve, but being $8\frac{1}{2}$ inches long it was enough to cover half the curve which, as stated at the end of the discussion on the evolute, is $7\frac{1}{2}$ inches long, leaving ends to enter clamping bars CD, as shown in figure 8. One of these saw-cuts is square across and the other is at 45°. These bars are placed over the saw-blade and are clamped to it by the two screws EF. An adjustable thrust-bar, G, can be inserted at any distance from the saw-blade, but always in a direction at 45° with the axes. Under the pressure of the thrust-bar G, which is equivalent to a line of tension as indicated by the two arrows *tt*, the saw-blade becomes curved in an elastica curve appropriate for the particular line of tension chosen. As this is placed further from the saw-blade the curvature becomes more uniform throughout. At an infinite distance the curve would be an arc of a circle. As the bar A is brought nearer to the saw-blade, the curvature at the end A becomes less, while that at the end B becomes more. The saw-blade is laid over an accurately drawn evolute and comparison is made with the curve. In this way practical identity of form is found when the distance of the bar away at the end A is $1\frac{1}{4}$ inches and at the end B is $3\frac{1}{4}$ inches, or very near these quantities.

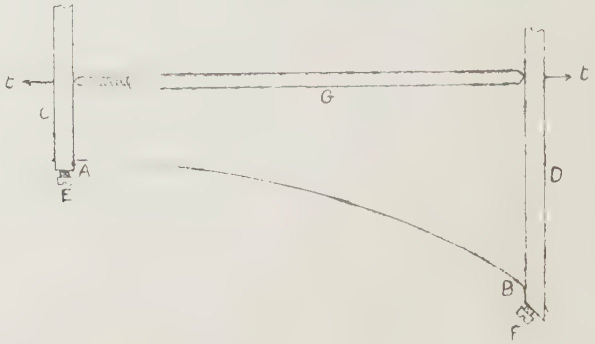


Figure 8.

Figure 9.

In the case of the evolute of the ellipse I did make a determination by experiment of the direction of the line of tension, and found it very closely parallel to the tangent of the curve at its minimum curvature. I am omitting a description of the experiment in order to economize space, but with some regret, as it was interesting and far from obvious. But a trial corresponding to that with the parent evolute can be described very shortly. We found in the discussion on the evolute that the angle made with the axes at the position of minimum curvature was 21°. I have accordingly cut off one bar at an angle of 21°, so that the hack-saw blade may be clamped to it there, and made a saw-cut at an angle of 69° in the other bar for a pinching screw to nip the blade at this angle. Then precisely the same process as that employed on the parent evolute (but using a rather thinner strip, as the saw-blade broke), a strut distant 1·7 inch and 0·7 inch from the ends and 0·35 inch from the point of minimum curvature, as shown in figure 9, also gave a curve barely, if at all, distinguishable from the evolute.

Figures 8 and 9 both illustrate also the fact that an elastica curve is the form of a thin arch in nearly unstable equilibrium supporting a load of water up to the level of the line of tension.

THE DESIGNING OF NON-SPHERICAL SURFACES

A discussion held by the Optical Group 16 *April* 1943

Instructor-Captain T. Y. BAKER. All optical design is based on the image formation associated with the paraxial equations. These are, for a single surface, $n'/v-n/u=(n'-n)/r$ and $y'/y=nv/n'u$. They imply that all the image-forming rays starting from the object point pass exactly through the image point. This is not generally true, but only approximately so, for rays that lie infinitesimally close to the axis. In other words, it is true only for a tiny field and a tiny aperture. For finite fields and finite apertures the rays cover a patch of finite size on the image plane, and it is the designer's job to see that that patch is reduced to something small enough for the duty the instrument has to fulfil.

Consider for a moment an image point on the axis when the cone of image-forming rays is of finite aperture. Any ray cutting the axis at an angle $\tan^{-1}t$ falls short of the paraxial image position by the amount of the longitudinal spherical aberration, which can be written At^2+Bt^4+ etc. The coefficient A is the coefficient of first-order spherical, B that of second-order and so on. It can be shown that if the surface is spherical

$$A=\tfrac{1}{2}v(1-kM)^2(1-k^2M)/(1-k)^2,$$

where k stands for n'/n, whilst M, the linear magnification, equals $nv/n'u$. It will be seen that unless the refractive indices or the radius of the curve are altered, the value of A is definitely fixed for any given image position and the designer can do nothing about it. The value of A is zero at three positions : (1) at the surface where $v=0$; (2) at the centre of the curve where $M=1/k$; and (3) at the aplanatic point where $M=1/k^2$. The value of A is negative only between the surface and the aplanatic point, and even within this range the amount of over-correction becomes zero at the centre.

If we take two spherical surfaces forming the boundary of a thin lens, refractive index n, we get for the value of the coefficient of spherical aberration

$$A=\frac{f(1-M)^2}{2n(n+2)}\left[\{(n+2)(1-M)C/F-(n+1)(1+M)\}^2-\tfrac{1}{4}n^3(1+M)^2\right.$$
$$\left.+\tfrac{1}{4}n^3(n+2)(1-M)^2/(n-1)^2\right],$$

where F is the power and f the focal length of the lens and C the mean curvature of the two surfaces. With a positive lens the factor outside the square bracket is positive, and of the three quantities inside the bracket the first and last are necessarily positive and the middle one negative. The first of these three quantities depends on the shape of the lens, and although it can never be made negative, it can be made zero if the lens is of suitable shape. Of the two terms remaining, sometimes the negative term and sometimes the positive term is the greater, so that there are some image positions where it is possible to get over-correction, others where it is not. But the amount of the over-correction is only very small. For a glass of refractive index 1·5, the maximum amount of over-correction occurs at $M=1{\cdot}10$ and gives a value $A=-0{\cdot}00225f$. For $n=1{\cdot}7$, $M=1{\cdot}17$ and $A=-0{\cdot}00712f$. The values of the coefficient A are shown in figure 1 as ordinates for five separate values of n from 1·5 to 1·7.

For a series of surfaces of radii r_1, r_3, r_5, etc., separating media of refractive indices n_0, n_2, n_4, etc., the values of the successive coefficients of spherical aberration are connected by the equations

$$\begin{aligned}(A_2/n_2)-(A_0/n_0)M_1{}^4k_1{}^2 &= \tfrac{1}{2}(v_1/n_2)(1-k_1M_1)^2(1-k_1{}^2M_1)/(1-k_1)^2\\(A_4/n_4)-(A_2/n_2)M_3{}^4k_3{}^2 &= \tfrac{1}{2}(v_3/n_4)(1-k_3M_3)^2(1-k_3{}^2M_3)/(1-k_3)^2\\\cdots\cdots &= \cdots\cdots\end{aligned}$$

If the optical system consists of a series of lenses separated by air, then $k_1k_3=k_5k_7=\ldots=1$. Now the first coefficient A_0 may be taken to be zero, and when we proceed to eliminate the intermediate (A/n)'s we get for the final (A/n) the sum of all the quantities on the right-hand sides of the equations, each multiplied by a positive quantity. We have seen that the right-hand sides are much more likely to be positive than negative, and at the best the negative values are only small. Hence it follows that it is almost impossible, with positive lenses alone, to get anything but an under-corrected system. It is only the negative lenses and the prism blocks that enable a system to be cleared of first-order spherical.

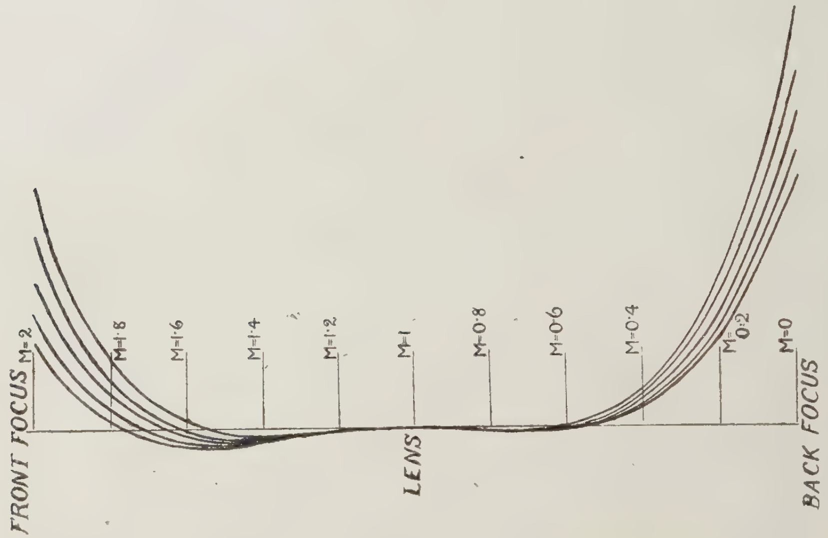


Figure 1.

A somewhat similar remark can be made with regard to the removal of astigmatism. For a single thin lens the primary foci lie on a curve whose curvature is $F(3k'+1/n)$ and the secondary foci on another curve whose curvature is $F(k'+1/n)$, where k' is the coefficient of astigmatism, and can be written

$$k'=\frac{n+2}{n}\left\{\frac{vv'F}{v-v'}\left(\frac{C}{F}\right)-\frac{n+1}{n+2}\left(\frac{v+v'}{v-v'}-\frac{vv'F}{v-v'}\right)\right\}^2 \\ -\frac{\frac{1}{4}n}{n+2}\left(\frac{v+v'-vv'F}{v-v'}-\frac{n+2}{n}\right)^2+\frac{\frac{1}{4}n^2}{(n-1)^2}\left(\frac{vv'F}{v-v'}\right)^2,$$

v and v' being the distances of the image and stop planes beyond the lens. Here again the coefficient consists of two positive and one negative term. The first involves the shape of the lens and can be made zero. The values of the other two depend on the positions of the image and stop planes. These values are set out in a diagram for a refractive index 1·52 (figure 2). The diagram is used by drawing a line through A corresponding to the image position and another through B corresponding to the stop position. Curves of constant k' are drawn in the diagram (they are all hyperbolas) and the appropriate value of k' read off from the point where the A and B lines intersect. Since values of C/F numerically greater than 4 lead to lens forms that are steeply meniscus, these values of the lens shapes are taken to be the limits of what is practically useful. The astigmatism curves are consequently modified at the places where they cross the two lines through C corresponding to these values. It will be seen from the diagram that the area covered by curves of negative astigmatism is only a fraction of the whole area. It must also be remembered that the " best form " of lens cannot always be used, and that any departure from it leads to increased positive or diminished negative astigmatism. On the whole, I think it can be stated that it is easier to get positive than negative coefficients, though the trouble is not so pronounced as in the case of spherical aberration. Composition of astigmatism for a succession of lenses merely involves the addition of the curvatures of the primary and secondary curves, so that the final curvatures are $3\Sigma F_1k_1'+\Sigma F_1/n_1$ and $\Sigma F_1k_1'+\Sigma F_1/n_1$. In order to obtain flatness of field and freedom from first-order astigmatism, we must have $\Sigma F_1k_1'=0$ and $\Sigma F_1/n_1=0$. The second condition is that the Petzval curvature must be zero. In the majority of optical systems this curvature is necessarily positive and the best compromise that can then be made is to reduce to zero the curvature of a curve half-way between the primary and secondary. This requires a final negative astigmatism, and to get this the designer has to take every advantage he can of those lenses so situated with respect to the image and the stop that they will give negative contributions to the final sum.

I have tried to make it clear that as regards spherical aberration, and, to a less degree, as regards astigmatism, spherical refracting surfaces are a handicap to the designer, and he very soon comes up against the limit of what is possible in field and aperture for any given lens system. The justification for using spherical surfaces is that they are easy to make, easy to test and easy to assemble. But for the designer who wants to extend the paraxial image formation both in field and aperture they are a hindrance. The paraxial equations depend only on refractive index and axial curvature, for a single surface ; on separations also where more than one surface is involved. They take no account of variations of curvature off the axis. The equation of a surface of revolution round the axis, and having a radius of curvature r on the axis, can be written generally

$$y^2=2rx-px^2+qx^3-\text{ etc.}$$

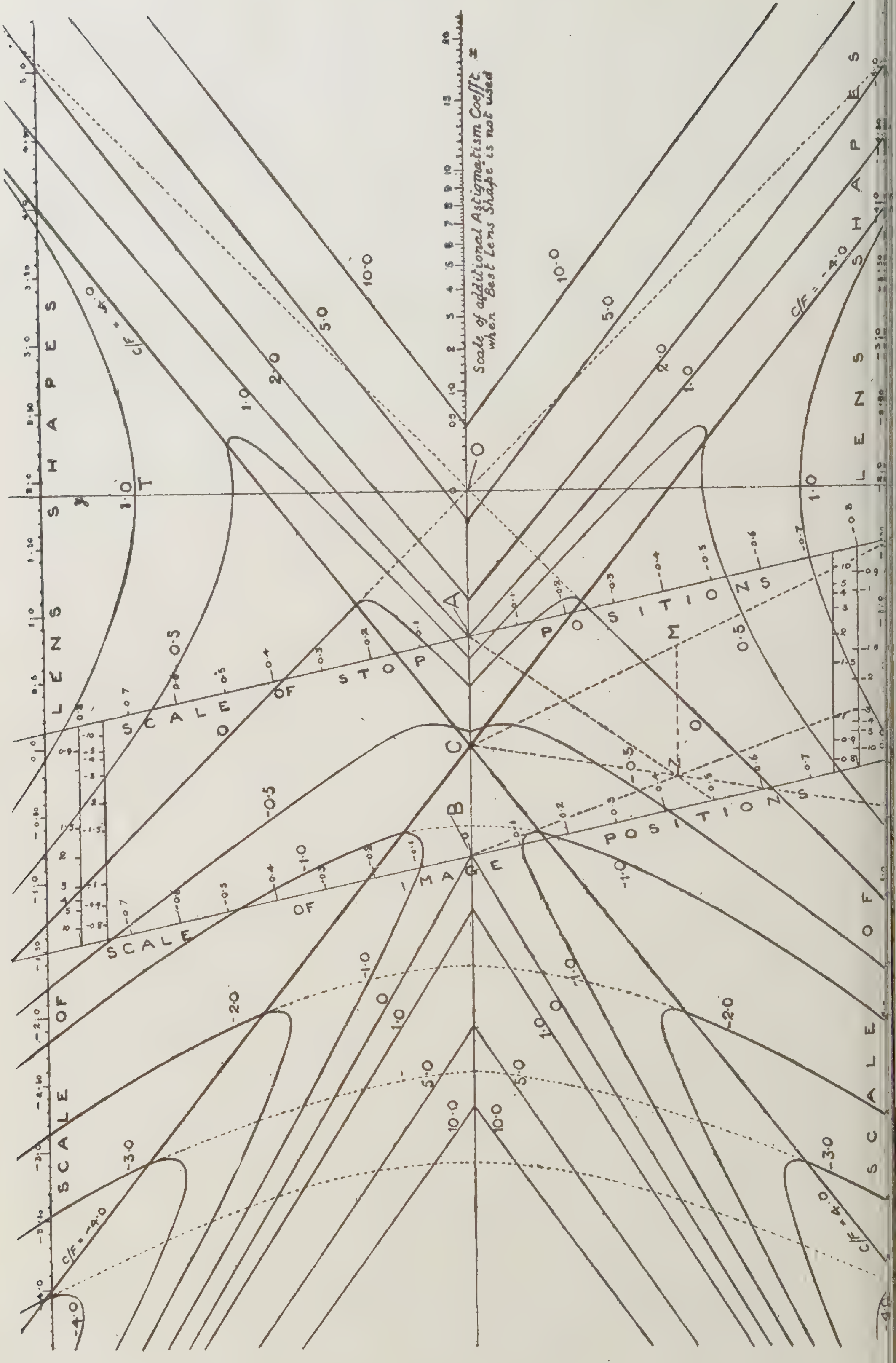
If $p=1$ and subsequent coefficients are zero the curve is a sphere. If $p=0$ under the same conditions the surface is a paraboloid. For negative values of p the surfaces are hyperboloids, for positive values ellipsoids. The value of p will affect the first-order aberrations. The value of q affects the aberrations of the second order, and so on. For reasons to be explained subsequently, I think it desirable to confine our non-spherical surfaces to those whose equations can be expressed generally as $y^2=2rx-px^2$.

This curve is a conic with one axis along the optical axis. It cuts the axis at $x=0$ and $x=2r/p$. The centre of the conic is, therefore, at $x=r/p$, and the two semi-axes are r/p and $r/\sqrt{p}$. If p is positive and greater than unity, the short axis of the ellipse lies along the optical axis. If less than unity, the major axis lies along this line. For negative values of p the real axis of the hyperbola lies in the optical axis. When p is greater than one the curve becomes steeper away from the axis ; for values less than one the curve is flatter at the edges.

For a single non-spherical surface we obtain

$$A=\tfrac{1}{2}v\{(1-kM)^2(1-k^2M)+(p-1)(1-M)^3k^2\}/(1-k)^2,$$

which shows that no matter what the position of the image it is always possible (except close to the surface) to obtain over-correction of first-order spherical.


Scale of additional Astigmatism Coefft. x when Best Lens Shape is not used
SCALE OF LENS SHAPES
SCALE OF LENS SHAPES
SCALE OF STOP POSITIONS
SCALE OF IMAGE POSITIONS
C/F = 4.0
C/F = -4.0
C/F = -4.0
C/F = 4.0
O
A
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y
10.0
5.0
2.0
1.0
0.5
-0.5
-1.0
-2.0
-3.0
-4.0

Figure 2.

A similar state of affairs holds in respect of astigmatism. If C_1 and C_2 are the curvatures of the primary and secondary fields,

$$C_1/n'=3\left\{\frac{vv'}{n'(v-v')}\right\}^2 P+\frac{n'-n}{nn'r},$$

$$C_2/n'=n\left\{\frac{vv'}{n'(v-v')}\right\} P+\frac{n'-n}{nn'r},$$

where

$$P=(n'/n)^2(n'-n)\left\{\frac{1}{r}-\frac{n+n'}{n'v}\right\}\left(\frac{1}{v'}-\frac{1}{r}\right)^2+\frac{(p-1)(n'-n)}{r^3}.$$

The inclusion of the non-spherical part of P, viz. $(p-1)(n'-n)/r^3$, makes it possible to vary the astigmatism coefficient at will at any surface without alteration of stop or image positions.

From what has been said above it is evident that the possibility of using non-spherical surfaces gives the optical designer much more scope. For every lens in the system he has now, instead of the single parameter involved in the bending of the lens, two additional ones in the "non-sphericities" of the two surfaces. The first-order aberrations are directly affected, and the results are seen in the changed curvatures in the neighbourhood of the axis of the spherical-aberration diagram and of the astigmatism curves. The exact shapes of these curves for the outer parts of the field and aperture must be dealt with by ray tracing. It is largely on this account that I advocate the limitation of the non-spherical surfaces to spheroids of revolution about the optical axis. The numerical computation will then involve solution of quadratic equations, but nothing of higher degree. A quadratic can be solved with five- or six-figure accuracy with fair rapidity, particularly if use is made of certain logarithmic tables; but the solution of cubics or quartics by successive approximation would be a laborious undertaking. There is an additional point that certain geometrical properties of the conics used as the generating curves might conceivably lead to mechanical methods of manufacture.

If we look at the enormous amount of computation work that has been done in the past sixty or seventy years on eyepiece systems, on photographic lenses and on microscope objectives, all containing nothing but spherical surfaces, it becomes clear that designers have been very severely handicapped by the lack of parameters with which to combat aberrations. Non-spherical surfaces provide these parameters and open up fields of investigation that badly need exploration.

Prof. L. C. MARTIN. It is not wise to assume that the design of optical systems employing co-axial surfaces of revolution must necessarily develop along the same lines as those used for spherical surfaces. In both cases, analytical methods offer much help in the general planning of systems free from the primary aberrations, but the removal of higher aberrations in such systems as photographic lenses has hitherto called for full studies of sufficient sets of ray paths to give a fair idea of the balance of the residual errors. This has been possible because the trigonometrical work has been comparatively rapid, and the sequence of steps can be readily memorized.

When aspheric surfaces are contemplated, the simple trigonometric process must be discarded. However, if the surfaces are such that they can be generated from curves of the second degree, ray-tracing is still a reasonable proposition involving nothing more difficult than the setting up and solving of the quadratic equation derived from the simultaneous equations of ray and surface. If it were possible to restrict designs to such surfaces, valuable degrees of freedom would be secured without a major enhancement of difficulties in the paper work of design.

There are, however, two important difficulties. Firstly, no effective means seem to exist at the present time for grinding and polishing prescribed conic-section surfaces by mechanical mechanisms ; that is, with the necessary degree of accuracy. (They can be produced by very careful hand-work, but the process would be costly, out of all proportion to the making of accurate spherical surfaces.) Secondly, the accurate testing of individual aspheric surfaces for figure is difficult and involves either the development of elaborate null tests or the none-too-easy preparation of master surfaces for use in some interferometric method of testing. A spherical surface can usually suffer small changes of radius without appreciably upsetting the performance of a system, but such a change might minimize the effectiveness of an aspheric test.

Where efforts have been made to produce aspheric systems (Schmidt cameras, Baker cameras, etc.) there has been a tendency to make the figuring dependent as much as possible on tests on the final image, rather than on tests applied with great precision to individual surfaces. If, then, the figuring of a certain surface removes the spherical aberration (say) of all orders in such a way as to produce the best possible image, this surface will, in general, be anything but " second degree ".

It was with this difficulty in mind that I recently commenced the study of two aspheric singlet lenses. In each case it was imagined that the figures of the lens surface were such as to remove spherical aberration completely (with certain other aberrations). The necessary shape was investigated (partly analytically, partly by trial and error), and it was then desired to test its performance by ray-tracing.

(1) *Case of surface equation in Cartesian co-ordinates*

The combination of the ray equation with the surface equation will, in such a case as at present contemplated, yield an equation of high degree, which cannot be directly solved, but a solution can be arrived at by successive approximations, for example by using Newton's method. When the co-ordinates of the point of intersection have been found, it is easy to write down the equation to the normal, and hence to calculate the angle of incidence of the ray. Also the radius of curvature at the point can be found, i.e., the radius for the " tangential fan " trace. The sagittal radius will be the interval, measured along the normal, from the curve to the intersection of the normal with the axis.

Various special methods of ray tracing in such cases have come to the notice of the writer, and it is possible that in some cases the equation to the curve should at once be put into terms of Cartesian co-ordinates. However, a preliminary discussion of some other possibilities may be of use.

(2) *Case of surface equation in polar co-ordinates*

In some cases the surface equation will be given in terms of polar co-ordinates referred to an origin in the axis of symmetry,

$$r=f(u), \qquad \ldots\ldots (1)$$

and since the angle i between the normal to the surface and the radius vector is given by the familiar equation

$$\tan i=\frac{1}{r}\frac{dr}{du}, \qquad \ldots\ldots (2)$$

it is, in theory, very simple to trace any ray from the origin through the surface. But in practice the theory of aplanatic mirror and lens systems developed on lines similar to those used by Chrétien will lead to series of the form

$$r=r_0+Au^2+Bu^4+Cu^6+Du^8+\text{ etc.}, \qquad \ldots\ldots (3)$$

whence the series for dr/du is

$$\frac{dr}{du}=2Au+4Bu^3+\text{ etc.} \qquad \ldots\ldots (4)$$

The possibility of making an accurate numerical estimate of the angle of incidence in the truly aplanatic lens will therefore depend on the satisfactory convergence of such series ; this will usually be maintained only for modest values of u, but, even so, there are useful possibilities. In practice, the numerical work will at once reveal the state of convergence.

Suppose that the polar equation of the curve AP with respect to the origin O (figure 1) is given by (3) above. A ray QP from an axial point Q and having an inclination θ to the axis is now to be traced through the surface. If OS, length p, is the perpendicular from the origin to this line, and the angle $Q\widehat{O}S$ is α, the polar equation to the line is

$$r\cos(u+\alpha)=p. \qquad \ldots\ldots(5)$$

In some cases (1) and (5) might be taken as simultaneous equations in u and solved fairly easily, thus obtaining r and dr/du, but sometimes a more practicable procedure will be to refer the polar equation (3) to the origin Q.

Let the distance OQ be d and the distance QA be ρ_0, then

$$\rho_0=r_0-d. \qquad \ldots\ldots(6)$$

Let the new equation be

$$\rho=\rho_0+M\theta^2+N\theta^4+P\theta^6+R\theta^8+\text{ etc.} \qquad \ldots\ldots(7)$$

It is now necessary to calculate M, N, etc., in terms of A, B, etc. Several methods may be employed. Those noted here, though not the most elegant, yield results which may be incidentally useful in other connections.

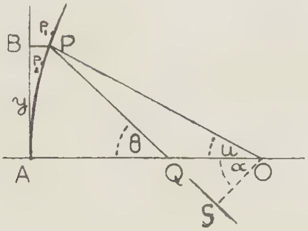

Figure 1.

First, the paraxial radius of curvature r_p is easily found to be

$$r_p=\frac{r_0}{1-2A/r_0}=\frac{\rho_0}{1-2M/\rho_0}, \qquad \ldots\ldots(8)$$

whence

$$M=A\frac{\rho_0^2}{r_0^2}+\frac{(r_0-\rho_0)\rho_0}{2r_0}. \qquad \ldots\ldots(9)$$

or

$$M=\frac{\rho_0}{r_0}\left(A\frac{\rho_0}{r_0}+\frac{d}{2}\right). \qquad \ldots\ldots(10)$$

Proceed now to calculate the horizontal gap BP between the surface and a plane tangential to it at the axial point. If the curve were a circle of radius r_p, let the gap be x at a height y, then

$$y^2=2r_px-x^2.$$

Solving this as a quadratic in x and expanding by the binomial theorem we obtain, in a few steps,

$$x=\frac{y^2}{2r_p}+\frac{y^4}{8r_p^3}+\text{ etc.};$$

but the true curve representing the surface will depart from the circle by an amount (say)

$$py^4+qy^6+\text{ etc.},$$

so that, including terms only up to y^4, we may write

$$\text{BP}=\frac{y^2}{2r_p}+y^4\left(\frac{1}{8r_p^3}+p\right)+\text{ etc.} \qquad \ldots\ldots(11)$$

Introduce now

$$\text{BP}=r_0-r\cos u \qquad \ldots\ldots(12)$$

and

$$y=r\sin u. \qquad \ldots\ldots(13)$$

Substituting from (12) and (13) in (11) and expanding both sides of the equation as power series in u, it is possible to obtain an expression for B. Some intermediate steps are

$$r\cos u=(r_0+Au^2+Bu^4+\text{ etc.})\left(1-\frac{u^2}{2!}+\frac{u^4}{4!}-\text{etc.}\right)$$

$$=r_0+\left(A-\frac{r_0}{2}\right)u^2+\left(B-\frac{A}{2}+\frac{r_0}{24}\right)u^4+\text{ etc.} \qquad \ldots\ldots(14)$$

Also

$$y^2=\{r_0^2+2Ar_0u^2+(A^2+2r_0B)u^4+\text{ etc.}\}\left(u^2-\frac{u^4}{3}+\text{ etc.}\right)$$

$$=r_0^2u^2+\left(\frac{6Ar_0-r_0^2}{3}\right)u^4+\text{ etc.},$$

and

$$y^4=r_0^4u^4+\text{ etc.}$$

When these values are inserted in (11) we obtain

$$-\left(A-\frac{r_0}{2}\right)u^2-\left(B-\frac{A}{2}+\frac{r_0}{24}\right)u^4-\text{ etc. }=\frac{r_0^2u^2+\dfrac{6Ar_0-r_0^2}{3}u^4}{2r_p}+r_0^4u^4\left(\frac{1}{8r_p^3}+p\right). \qquad \ldots\ldots(15)$$

Equating coefficients of corresponding powers of u we obtain

$$-\left(A-\frac{r_0}{2}\right)=\frac{r_0^2}{2r_p},$$

which agrees with (8) above, or

$$A=\tfrac{1}{2}\left(r_0-\frac{r_0^2}{r_p}\right). \qquad \ldots\ldots(16)$$

Further, we obtain

$$\frac{6Ar_0-r_0^2}{6r_p}+\frac{r_0^4}{8r_p^3}+pr_0^4=-B+\frac{A}{2}-\frac{r_0}{24},$$

and after some simplification, using the value of A in (16),

$$B=\frac{5}{24}r_0-\frac{7}{12}\frac{r_0^2}{r_p}+\frac{r_0^3}{2r_p^2}-r_0^4\left(\frac{1}{8r_p^3}+p\right). \qquad \ldots\ldots(17)$$

The formulae (16) and (17) give the first three terms in the polar equation of a surface which has received a certain specified amount of "fourth-power" figuring.

The equation corresponding to (17) could have been written in terms of ρ_0 and N, i.e.,

$$N=\frac{5}{24}\rho_0-\frac{7}{12}\frac{\rho_0^2}{r_p}+\frac{\rho_0^3}{2r_p^2}-\rho_0^4\left(\frac{1}{8r_p^3}+p\right).$$

Eliminating p from these last two equations, we obtain

$$N=\rho_0^4\left\{\frac{5}{24}\left(\frac{1}{\rho_0^3}-\frac{1}{r_0^3}\right)-\frac{7}{12r_p}\left(\frac{1}{\rho_0^2}-\frac{1}{r_0^2}\right)+\frac{1}{2r_p^2}\left(\frac{1}{\rho_0}-\frac{1}{r_0}\right)+\frac{B}{r_0^4}\right\}. \qquad \ldots\ldots(18)$$

The foregoing result was checked algebraically in the following way. Adopt the intermediate series,

$$\left.\begin{aligned} r&=r_0+A\sin^2u+B'\sin^4u+\text{ etc. . . }(a),\\ \rho&=\rho_0+M\sin^2\theta+N'\sin^4\theta+\text{ etc. . }(b),\end{aligned}\right\} \qquad \ldots\ldots(19)$$

in which, using the known series,

$$u=\sin u+\frac{\sin^3 u}{3!}+\text{ etc.}$$

and

$$\sin\theta=\theta-\frac{\theta^3}{3!}-\text{ etc.},$$

we easily find that the values of A and M will be identical with those in (3) and (7), but the coefficients of higher powers differ ; thus

$$\left.\begin{aligned} B'&=B+\frac{A}{3} \;\ldots\; (a),\\ N&=N'-\frac{M}{3} \;\ldots\; (b). \end{aligned}\right\} \qquad \ldots\ldots(20)$$

Writing $r\sin u=\rho\sin\theta$

and $r^2=\rho^2+2\rho d\cos\theta+d^2,$

both sides of the last equation could be expanded in terms of θ. Equating coefficients of corresponding terms then gave

$$N'=\frac{2A\rho_0M}{r_0^2}-\frac{3A^2\rho_0^4}{2r_0^5}+\frac{B'\rho_0^4}{r_0^4}-\frac{M^2-dM-\rho_0 d/4}{2r_0}, \qquad \ldots\ldots(22)$$

and on completing the steps involved in (20), the result agreed algebraically. A numerical check was also tried in a particular case.

(3) *Coefficients of higher terms*

In some cases at least, where the angle is very small and where the point Q is near the centre of the curve, the calculation of N will suffice to give the value of ρ and the angle of incidence with sufficient accuracy, but in other cases the values of higher terms will be required. The approximate position of the intersecting point will be known. Select, then, two points, P_1, P_2, a little above and below P respectively, defining these by assumed values of the angles $\widehat{AOP_1}$, $\widehat{AOP_2}$ (u_1 and u_2). This allows the exact calculation of OP_1 and OP_2 from the given formula of the polar curve. Having obtained these values, the corresponding values QP_1, QP_2 are obtained trigonometrically, also θ_1, θ_2. We thus obtain two simultaneous equations for P and R in the series (7), i.e.,

$$\left.\begin{aligned} \rho_1&=\rho_0+M\theta_1^2+N\theta_1^4+P\theta_1^6+R\theta_1^8 \;\ldots\; (a),\\ \rho_2&=\rho_0+M\theta_2^2+N\theta_2^4+P\theta_2^6+R\theta_2^8 \;\ldots\; (b), \end{aligned}\right\} \qquad \ldots\ldots(23)$$

and thus their numerical values can be found. The resulting equation, terminating with the term in θ^8, will represent a curve which has (in relation to the true curve) the same axial curvature and the same " y^4 " figuring ; moreover, it will cut the true curve at two points on either side of the ray intersection. If numerical inaccuracy were not to be feared it would be possible to make these two points very close indeed to the expected point of ray intersection, when the calculated curve would (in the limit) touch the true curve, but if the new equation is to serve for a number of rays from the new origin it will be better to give the points P_1, P_2 a reasonable interval.

(4) *Method for large angles of incidence*

In cases when the angle between the ray and the axis is large, the above procedure may not be convenient, and an alternative procedure must be used. Let APR (figure 2) be the principal section curve of the surface, given in polar co-ordinates with respect to the axis AQSO and the origin O. The incident ray TQ is defined by the point Q, and the angle u_0 included between it and the axis, but the point of its intersection with the curve is only approximately known. Let this point (estimated by any convenient method, possibly a careful graphical ray trace) be P, a point on the curve defined by an assigned numerical value of u. First calculate i, the angle between OP and the normal PS, from the equation

$$\tan i=-\frac{1}{r}\frac{du}{dr}, \qquad \ldots\ldots(24)$$

and obtain the radius of curvature ρ_c at the point P from the usual formula,

$$\rho_c = \frac{\left\{r^2+\left(\frac{dr}{du}\right)^2\right\}^{3/2}}{r^2+2\left(\frac{dr}{du}\right)^2-r\frac{d^2r}{du^2}}. \qquad \ldots\ldots (25)$$

This radius will be useful if a tangential fan of rays is to be traced through the surface.

The tracing of the ray TQ can now be referred to the auxiliary axis PS, treating the case by the usual ray-tracing methods for a circular curve of the above radius, for which

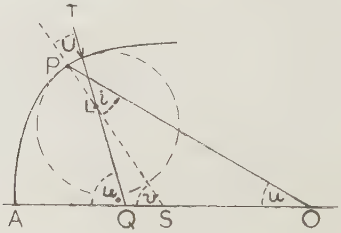

Figure 2.

the initial data required are the angle $\widehat{PLT}$, (U), and the distance PL, (L) ; as seen in the diagram, L is the crossing point of the ray TQ and the normal PS. We have

$$v=i+u \qquad \ldots\ldots (26)$$

and

$$U=u_0-v. \qquad \ldots\ldots (27)$$

Also

$$L=\mathrm{PS}-\mathrm{LS} \qquad \ldots\ldots (28)$$

$$=\frac{r\sin u}{\sin v}-\frac{\mathrm{QS}\sin u_0}{\sin U}$$

$$=\frac{r\sin u}{\sin v}-\frac{\{\mathrm{QO}-(r\sin i)/\sin v\}\sin u_0}{\sin U}. \qquad \ldots\ldots (29)$$

The above formulae will give positive signs to the quantities in the figure, from which the sign convention may be inferred. The final stages of the ray tracing are obvious, and need not be set out here. It may be noted in passing that the value of PS will be the sagittal radius of the curve at the point P, and if the actual ray-crossing point turns out to be very close, the above radius will serve in tracing a sagittal fan associated with TQ.

(5) *Rough method*

In some cases the height of incidence of the ray will be small, so that the separation between the aspheric curve and the circle having the same radius of curvature at the axis will be exceedingly small and calculable (from the polar equation of the curve referred to its centre of curvature) with sufficient accuracy even if only the term in u^4 is included, the coefficient being obtained from (18) above by putting $\rho_0=r_p$. Or, possibly, the figuring constant may already be known.

Ordinary methods allow the calculation of the point P (figure 3), where the incident ray cuts the above osculating circle. This in turn yields the distances PN, the perpendicular from P to the axis, and ON, whence OP and the angle $\widehat{POA}$ (i.e. u) can be determined. This permits the calculation of PM, the normal distance between the point P and the curve. The true crossing point of the ray and the curve is, however, R ; if the gap is very small, MR may be taken as an element of the tangent at Q. The determination of u further permits the calculation of i, the angle between the radius vector and the normal to the curve at the point very close to M where OP cuts the curve. The normal PM cuts the axis at S when produced; thus, if $\widehat{MSA}$ is v,

$$v \approx i+u \quad \text{(very nearly)}$$

and

$$\widehat{RPM}=v-u_0$$

(remember that v is numerically negative in the above diagram).

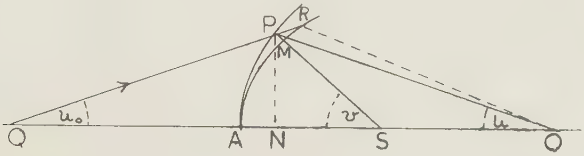

Figure 3.

Now

$$MR=PM \cot \widehat{RPM},$$

and if we put

$$\delta u=\frac{MR \cos i}{r}$$

we shall obtain a correction δu to be added to the original estimate of u which should give a fairly accurate value of the angle $\widehat{ROA}$, from which the true angle of incidence of the ray can readily be obtained.

(6) *Numerical trials of ray-tracing methods*

In designing a singlet lens intended to be completely free from spherical aberration, the following polar equation was used for the back surface :

$$r=0{\cdot}49310\{1-0{\cdot}30789u^2-2{\cdot}1240u^4-6{\cdot}712u^6-51{\cdot}0u^8\}.$$

A ray to be traced through this surface had the following data :—

$$u_0=-\ 4^\circ \quad 40' \quad 24''{\cdot}9$$

$$AQ=-0{\cdot}411670.$$

Refractive index of glass $=1{\cdot}6200$.

Of course the hope of estimating the performance of the ideal lens rests entirely on the adequacy of the proposed equation. Once that is settled, different ray-tracing methods should yield the same result, but in view of the approximations involved in all of them it is interesting to study the relative results.

The first methods used were of the type of (5) above (the " Rough Method "). Five figures were used in the tracing, which was done with the aid of a Brunsviga machine and Peters' six-figure table of natural functions. Obvious inaccuracies were soon found to be present, and careful six-figure work was then tried. The results were then checked by methods (1) and (2), using seven figures, and logarithmic calculation (the seven-figure log tables of Dupuis were employed). Results were as follows (for the final angle between ray and axis) :—

Method 5, Six-figure (machine),	11° 36′ 9″
,, 3, Seven-figure (logs)	11° 36′ 4″·9
,, 4, ,, ,, ,,	11° 36′ 6″

It is clear that the tracing of a ray to " one-second " accuracy through such a complicated surface is not quite assured by the methods described, and eight-figure work is indicated.* The same difficulties would not, of course, be encountered in simpler surfaces, but the actual shapes of figured surfaces may be quite complex.

My personal view is that while ray-tracing may easily be employed with advantage to investigate the effect of a " few-wave-lengths-deep " figuring on pre-designed spherical surface systems (using methods similar to (5) above), the general task of ray-tracing through deeply aspheric surfaces is so laborious that it will not be extensively employed, unless means can be found greatly to simplify it.

Mr. J. H. JEFFREE. I agree that any method a designer fancies for computing Seidel aberrations of spheres could be extended to non-spherical surfaces, though I had hoped also to see a convenient general form for ray tracing through them put forward.

The use of such surfaces in meeting every-day requirements of corrected systems may be well worth while in certain cases, because of the wider range of possibilities from which they allow the designer to choose. For instance, difficulties of various kinds arise in developing photographic anastigmats in the well known optical plastics, methyl methacrylate and polystyrene, because of their inconvenient optical properties for this purpose—roughly parallel to those of hard crown and dense flint glasses. But in some of the less conventional types of system which non-spherical surfaces make possible, these difficulties are much less serious than in the more usual spherical types, so that they can, in effect, be circumvented.

Most of such advantages can be gained without leaving the range of surfaces based on conic sections. Sometimes more complex forms are desirable, but they need, of course, more labour in computation and are harder to realize in practice.

Mr. T. SMITH. May I first congratulate the Group on its success in attracting so large a company to a meeting for the discussion of this highly technical subject. It is most encouraging to find that so much interest is being taken in the problems of geometrical optics. If it is still true, as the late Lord Rayleigh once said, that writers in this subject are more numerous than readers, and that it seems almost impossible to induce one worker to read what another has written, there is apparently greater readiness to listen than to read.

This discussion helps us, I think, to realize why one worker may have comparatively little influence on another. The methods that have been described differ very considerably from one another, and I have no doubt that among the audience others have been thinking, as I have, that we are, fortunately, none of us compelled to use any one method of investigation. It is clear that a special method may be very appropriate for a particular group of problems, and much less convenient for another group. For example, Dr. Burch's method is very well adapted for systems which have some approach to spherical symmetry and for more complex systems built on a basis of this kind, but I think they are less suited to problems of a different kind than some other methods. Some thirty years ago I built up a system of investigation based on centres of curvature rather than on the poles of the refracting or reflecting surfaces, but experience taught me that it had disadvantages for numerical work. There was a tendency for the significant magnitudes to appear as the differences of much larger quantities. My final conclusion was that it is better to evaluate errors where they arise, that is in the neighbourhood of the surfaces themselves.

Another illustration of the variety to be found in ways of designing lenses is afforded by a comparison between Captain Baker's procedure and my own. You will note that he uses formulae involving intersection distances v and v', which are, presumably, found for each surface or each component lens. My preference is to compute constants for the system which are free from these variables, just as the familiar expressions for the power of a system or for the positions of the principal points are free from them, though both the power and the principal points can be calculated by means of equations which involve axis intersections ; and here, I may say, that ever since Airy and Coddington, both in this country and abroad, writers have tended to follow an unfortunate lead. The wrong variables have been adopted, with the consequence that algebraic theory has appeared to be more complicated than in fact it need be. An illustration may be taken from the theory of a single thin lens. It has one non-aberrational constant—its paraxial power F—and only two constants for the aberrations of the lowest order—the Petzval sum and the spherical aberration constant $(C+F)^2$, where C is the total curvature of the lens. It is customary to give six functions of the refractive index instead of these two quantities. When the surfaces are non-spherical there is an additional term in the spherical aberration constant, the value of which can be written down from the most elementary consideration.

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* A second approximation may also be called for in methods (3) and (4).

The method of designing systems, either with or without non-spherical surfaces, which appeals most strongly to me consists of three parts. The first part is concerned with the properties of thin lenses, and particularly with the changes that can be introduced in some groups of aberrations without affecting others. The means of controlling these groups must be properly understood. The second part consists in building up, from a number of elements having the characteristics of thin lens groups, a system with the properties required. When a combination capable of yielding these properties has been found, forms of construction for each thin lens group are determined by applying the knowledge obtained from the study of thin systems. In the third part the necessary substance is given to the component lenses, their separations and curvatures are altered by approximately determined amounts, and the work is completed by algebraic computation.

A final word may be added on algebraic versus trigonometrical calculations. It has sometimes been thought that the former are essentially approximate and not trustworthy, while the latter are unquestionably accurate. This is an error. The formulae used in trigonometric calculations are accurate, but the numerical work based on them is approximate. By taking a suitable number of figures, any errors of approximation may be reduced below a level of any importance. The results of algebraic computation are also approximate, and the magnitude of the outstanding errors depends on the method of calculation adopted. A method equivalent to expansion in an infinite series will normally be less accurate in numerical work than a trigonometrical method, but expansions of this type are valuable in deriving formulae to show how the different aberrations can be controlled. Other algebraic methods, however, which can be easily used for tracing both ordinary and skew rays through non-spherical surfaces, are not less accurate than trigonometrical methods. In fact it is easy to prove that if we wish to obtain very great accuracy we should trace rays algebraically and not trigonometrically.

Dr. E. H. LINFOOT. I should like to describe briefly a method which I have found useful in computing Baker astrographic cameras. I do not think that it is new, nor that it is necessarily the least laborious method, but it is simple, straightforward and adequate for the purpose.

First a word on the general layout of these cameras. They were first proposed in 1940 by Dr. James G. Baker of Harvard University, and their general appearance is shown in figure 1. M_1 is a large concave mirror, M_2 a smaller convex one of very nearly the same radius of curvature, while P is a correcting plate of the kind made familiar to astronomers by the Schmidt camera. That is to say, P is thin, nearly plane parallel, and aspherized on one surface. In the Baker cameras it is convenient to take this surface to be the one facing the mirrors. At least one of the mirrors must be aspherical.

These cameras, as James Baker pointed out in his original paper, are flat-fielded anastigmats whose performance over fields up to about 8° diameter is of the same high quality as that of the Schmidt camera. They can be made distortion-free if both mirrors, as well as the plate, are aspherized, but their distortion without this refinement is hardly such as to make it worth while, especially since the photographic plates need calibration in any case to allow for possible creeping of the emulsion during development.

The colour error of these systems is three or four times that of a Schmidt camera of the same effective aperture, and begins to be troublesome at apertures in the neighbourhood of $f/2$. In order to reduce it, the plate-profile is modified, by superposing slight convexity on the pure " 4th-power turn-up edge ", so as to make its greatest slope as small as possible. The colour-minimizing profile is attained when the deepest part of the curve is at about 0·866 of the full plate radius, but this figure assumes that the higher-order terms in the plate profile can be neglected. In fairly wide aperture systems they are not negligible, and a good working rule seems to be to bring the deepest part of the curve to about five-sixths of the full radius.

The added convexity, amounting as it does to the introduction of an additional very weak convex lens into the system, affects the off-axis errors of the system ; in particular, the Petzval curvature is changed and a little off-axis astigmatism introduced. Small changes in the positions and curvatures of the mirrors are needed to compensate this ; they can be determined up to Seidel accuracy by using the see-saw diagram of C. R. Burch, or otherwise.

Suppose now that we wish to design a B-type Baker system, that is to say one with a spherical secondary mirror. Having decided on a numerical aperture of, say, f/2, and on the position of the focal plane relative to the mirror M_1, we make a preliminary determination of the dimensions and figurings so as to annul the Seidel errors of the system. Then we proceed to the computational stage. We fix the *neutral zone* at five-sixths of the full aperture, as explained above, and determine the position of the focus F of rays parallel to the axis by tracing a ray inward through this zone of the plate, taking account of the asphericity on M_1 by using the Seidel approximation already determined.

F being determined, a meridional fan of rays is traced out from F at numerical apertures 0·01, 0·02, 0·03, ... up to 0·25 in the case of an f/2 system, and the intersections determined of these rays with the inner face of the plate P, which is taken as plane for this purpose. Let $h^{(1)}(\theta)$ denote the height at which this face is cut by the ray which leaves F at an angle θ with the axis. Then $h^{(1)}(\theta)/\sin\theta$ is found to be very nearly constant for the different values of $\sin\theta$ (since we have already ensured that the sine condition shall be satisfied up to Seidel accuracy by eliminating the primary coma in the preliminary design). The next step is to adjust the asphericity on M_1 so as to make $h^{(1)}(\theta)/\sin\theta$ constant as θ varies. This is done by a numerical integration procedure ; first the mirror slopes on M_1 at the points where the different rays meet it are put right without regard to the effect this change will have upon the *positions* of the points. Then the slope-corrections are integrated and the resulting adjustments to the figure of M_1 tabulated and added in. Sometimes the result is a sufficiently good approximation to the desired figure ; in a wide-aperture system, however, it is worth while to trace the rays again from the adjusted M_1 on to the plate, obtaining adjusted values $h^{(2)}(\theta)$ for the heights of their intersections, and make the further, much smaller adjustment to the figure of M_1 needed to bring $h^{(2)}(\theta)/\sin\theta$ to a constant value.

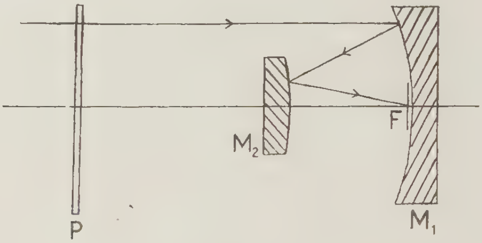

Figure 1.

The figure of M_1 being now determined, and also the height $h^{(2)}(\theta)$ and the directions of the rays between M_1 and the plate, we easily calculate the plate slopes needed to bring these rays out parallel to the axis after refraction by the plate. A numerical integration then gives the plate profile. The error due to the fact that the heights $h^{(2)}(\theta)$ have been calculated as intersections with the axial tangent plane to the plate surface instead of with the surface itself is negligible in these systems, since between the mirror M_1 and the plate the rays are nearly parallel to the axis of the system.

Further improvement of the design can probably best be made by constructing a prototype model and exploring the field with a microscope. The residual field-curvature can be balanced out by changing the radius of curvature of the spherical secondary. Strictly speaking the two asphericities should then be recomputed to regain exact satisfaction of the sine condition. In practice, it is sufficient to proceed by shifting the plate along the axis to annul the slight coma and figure it to give axial stigmatism. The higher coma and primary off-axis astigmatism which this procedure leaves uncorrected are harmless in amount.

Opener's reply. I agree with Mr. Jeffree that for photographic anastigmats made of transparent plastics most of the defects of image that must be tolerated when using spherical surfaces can be eliminated by utilizing non-spherical surfaces based on conic sections. I have already put forward a method of ray tracing for such surfaces, and hope shortly to follow it with a paper dealing with skew rays.

It is not easy to comment on Mr. Smith's remarks, since he gives very little detailed information respecting the methods he uses in designing lens systems, and one has to guess whether his " algebraic " method of controlling groups of aberrations without affecting other groups refers to first order only or to higher orders. He is a little hard, I think, in his denunciation of optical writers for following Airy and Coddington. After all, there are five first-order image defects which can be put into various forms. For a thin lens they depend on three quantities only. One of these is scalar, so that for a lens of unit power we can say that the five defects depend only on refractive index and lens shape. How one takes account of the lens shape is a matter of personal predilection. I myself prefer to deal with the mean curvature of the two surfaces. Mr. Smith has his Spherical Aberration Constant. Others favour treatment from the standpoint of the proportion of power put into the two surfaces. But whatever scheme is adopted there are only those two quantities that can affect the image defects, and every computer understands that in " bending the lens " he generally modifies every one of the five aberrations, or, in other words, that they are not independent of one another.

There is another point in which Mr. Smith comments on the procedure that I have advocated, namely the need for taking into account the image and stop positions in considering the aberrations produced by any refracting surface. Surely it is fairly obvious that this must be done in some form or other, since a change in, say, the stop-distance v' will involve a change in the part of the wave front to which the image-forming rays are confined.

I must refer also to Mr. Smith's remarks concerning the relative accuracy of the " algebraic " and " trigonometrical " methods of optical design. Surely Mr. Smith realizes that when optical computers consider that algebraic methods are not sufficiently accurate they are referring to calculations of the aberrations of the first order. If he can produce a method whereby aberrations of the second, third and higher orders can be computed as far as may be necessary, with less work than is involved in the trace of enough rays to show the nature of the image patch at various parts of the field, he will certainly make the designer's task easier. His comments do not indicate how this is done, and I cannot remember in any of his previous papers formulae which express higher-order aberrations in a shape suitable for rapid computation. After all, this is not very surprising. In the perfect instrument the final wave fronts emerge spherical over a wide angle of convergence. At intermediate stages the corresponding wave fronts are far from spherical. The two principal radii of curvature can be easily calculated, but to obtain the curvatures at the other parts of the wave front we need to know not only the curvatures for the principal ray but also their differentials as far as the seventh or eighth. They must not only be known but summed, and their interactions on one another computed. I think it is not surprising that on the whole computers prefer to trace rays through the system as the quickest means of discovering the shape of the whole wave front, and, therefore, the character of the image patch at various parts of the field.

Dr. Linfoot pursues a different method. The example he gives of his computations shows how to build up a non-spherical refracting surface which will correct the aberrations at a certain point of the field (in the case described at the centre). But a plate which corrects central definition will not necessarily be equally efficient at the edge of the field, and Dr. Linfoot's methods do not commend themselves as suitable for the optical designer who has to produce systems that are well corrected for large apertures and large fields at the same time.

Professor Martin's proposals for the consideration of non-spherical surfaces is to express their shapes in polar rather than Cartesian co-ordinates. There is certainly a great deal to be said in favour, but I think it will be found that the determination of the point where the ray cuts the surface, that is to say the solution of the equations $r\cos(u-\alpha)=p$ and $r=r_0+Au^2+Bu^4+Cu^6\ldots$, will be extremely troublesome.

Finally, I should like to make a suggestion. The ultimate practical test of all these proposed methods of optical design lies in the rapidity with which they produce results.

I propose, therefore, the following problem for a design that is badly needed. The eye-lens of an observing telescope suffers badly from the defect that it has a curved field, which is of the opposite curvature to that of the image formed by the objective and field lens. Consequently the focusing position is different at the edge of the field from what it is at the centre. Light transmission is of the highest importance, and the eye-lens *must* be made in the form of a cemented doublet. The only hope of improvement lies in the use of non-spherical surfaces. From manufacturing considerations, the contact surface must be spherical. Achromatism for power fixes the powers of the crown and flint components, and there remain to the designer three quantities that he can vary, viz., the curvature of the contact surface and the non-sphericities of the two outer surfaces. If he likes to pursue these beyond the first variation from spherical form (that is to say something more complicated than conics when expressed in Cartesian form), he has more than one variable available from each external surface. He has at all events three variables completely at his choice, and with them he must produce a lens which (when the light is taken backwards) has the following characteristics :

(1) unit equivalent focal length ;
(2) entrance pupil 0·3 in front of the lens ;
(3) a working aperture of f/3·5 ;
(4) a field of 66 degrees ;
(5) a field curvature of −0·1, that is to say opposed to the Petzval curvature of the lens ;
(6) achromatism of C and F lines.

An eye-lens of this type has frequently to be used with a graticule for laying on a target, and this graticule is commonly used away from the centre of the field. It would, therefore, be advantageous to design this lens so that the best definition is not at the centre of the field but at an angular radius of 10 degrees. If the advocates of the various design methods would tackle this problem *and show the whole of their paper work*, I think it might form a very useful subject for discussion at a future meeting of the Optical Group, say in a year's time.

OBITUARY NOTICES

FRANK FORSTER RENWICK

MR. F. F. RENWICK, Research Director of Ilford, Limited, who died at the age of 66 on 14 August 1943, was an outstanding figure in contemporary photographic science.

Renwick was educated at the City of London School and the Central Technical College of the City and Guilds of London Institute, where he studied under H. E. Armstrong in 1893 to 1896. Becoming A.C.G.I. in 1896, he was awarded an Institute Scholarship and the Leathersellers' Research Fellowship, which he held for a year.

He joined Ilford, Limited, as a chemist in 1898, and during the forty-five years which followed he made many important contributions to his subject. Blessed with a versatile mind and an exceptionally retentive memory, he was able to turn the experience of those years to such account that at the time of his sudden death he was at the height of his powers ; it was, indeed, during his last few years that he brought into being two of the most remarkable photographic products of the century (*Multigrade* and *Kryptoscreen*). His life-long interest in the measurement of photographic densities and in the problem of tone-reproduction led him in recent years to consider the desirable sensitometric properties of photographic paper. The fruits of this work are to be seen in present-day efforts to standardize the specification of paper contrast, and in the remarkable product, *Multigrade* paper—a paper of controllable contrast, for the introduction of which he was responsible little more than three years ago. Renwick also gave much time and labour to the quest for a photographic material which, by containing fluorescent substances, would have a greatly enhanced sensitivity to X rays. Ordinary photographic emulsions are sensitive to X rays, but absorb very little of the radiation and allow by far the greater part to pass through unused. Renwick's efforts were crowned with success after a prolonged study of x-ray fluorescence, when he discovered and put to use the remarkably powerful fluorescence, under x rays, of complexes of lead sulphate and barium sulphate. These complexes are used to form very efficient and relatively cheap x-ray intensifying media in *Kryptoscreen* paper and *Brytex* Intensifying Leaves.

From the earliest days his work brought him closely into touch with the Royal Photographic Society, before which he read many papers and took part in many discussions. He was largely responsible for the creation in 1920 of the Society's Scientific and Technical Group. In 1921 he was awarded the Progress Medal of the Royal Photographic Society and in 1936 was the Society's Hurter and Driffield Memorial Lecturer. In 1938 he was awarded the Péligot Medal of the Société Française de Photographie in recognition of his services to photographic science. Naturally enough he became President of the Royal Photographic Society (1927–1929), and here his capacity for clear thinking and his administrative ability still further increased the Society's debt to him.

That same administrative capacity stood him in good stead when, about the same period, the amalgamation of Ilford, Limited, with many other British photographic firms was taking place. To him was entrusted the difficult task of co-ordinating the laboratory staffs and services of the companies concerned, and this he did with such effect as to build a substantial research organisation on the foundations which he had helped to lay in the earlier years of the century. In this he was no doubt aided by experience gained during a brief interlude in the United States in 1922–1925 when he directed the Redpath Laboratory of the Du Pont Film Manufacturing Corporation at Parlin, New Jersey.

His outstanding integrity and devotion to his chosen work were obvious to all who knew him. Retiring by nature, and self-conscious even to a fault, he was none the less ready to champion a cause in which he believed ; and it was this dogged devotion to principle that won him some of what he would have regarded as his greatest successes. Undoubtedly, too, it led to his remaining at work till the last possible moment, which was within three weeks of his death.

He leaves a widow and four children of his former marriage—two sons, now in the Middle East, and two married daughters. L. V. CHILTON.

BARON HIRST OF WITTON

In the death of Lord Hirst there passed a great industrial pioneer. He created the General Electric Company, and its growth was due to his imagination and enterprise.

He was, of course, fortunate as well as far-seeing in choosing electricity and the production of electrical equipment as a field in which to occupy his genius ; for in the 80's, when he started, none could have foreseen the enormous part which electricity was to play in the social and industrial life of the world. But, like Ferranti, Crompton and Merz, his vision was usually ten years ahead of most other people's, so that his energetic planning, born of complete confidence in his own judgement, usually brought his enterprises to fruition at the right time.

He was a great believer in free enterprise ; but finance and profits were not his main object. Stable finance and quick decisions where money was concerned were fundamental requirements, but were no more than one of the essential means to the end. That end with Hirst was *industry*—dignified, strong and British. To him there were no essential antagonisms between industry and the Government, nor between so-called managements and workers. A community of interest binds them together so that mutual loyalty does not need any idealisms for its stimulation, but rests on the surer foundation of interdependence of interest.

Fortified by this simple and stalwart faith, his attitude to the Government was that, as an industry builder, he was a benefactor to the community ; to competitors his was a policy of *live and let live*, implemented by agreements and gentlemen's understandings ; as to the staff and workpeople in his own Company, were they not a family with one common object, and, therefore, to be treated as a family ?

Looking at industry today, when so many politicians seek its salvation by imposing on it a system of divided internal loyalties—when the amateur from without would seek to create, by administrative system, that which can only be the result of vital growth from within, it is refreshing to regard the plain and straightforward principles in human relationships which brought success during the whole of Lord Hirst's 50 years of active industrial life.

Hugo Hirst was born in Germany in 1863, but came over at the age of 16 to make England his country. In 1886 he joined Mr. G. Byng in the General Electric Apparatus Company—a small shop in Great St. Thomas Apostle. These were the days of the early electrical wonders—among which were Swan's electric lamp and the telephone. Courage and vision brought business in these to the little Company, and in 1889 its name was changed to the General Electric Company. A factory was bought in Manchester and manufacture was begun on a small scale until in 1893 the Peel Works in Salford was started. Then, in 1895, came the start of the Robertson Lamp Works in Hammersmith, and six years later the bold venture of a large electrical machinery factory in Witton, Birmingham. A smaller, but now historic, venture was the carbon works—which Hirst felt sure would be needed in the event of war, but he had to sacrifice £70,000 over several years in order to back his opinion. The war in 1914 fully justified it. Other large manufacturing enterprises followed—telephones at Coventry, cables at Southampton and turbines at Erith, as well as rapid expansion of the existing factories.

It was not an accident that the G.E.C. manufacturing units were distributed over so many centres. Hirst, and those who collaborated with him, saw advantages in this. The G.E.C. was never designed to be a highly centralized organization, but rather a group of suitably located but dispersed manufacturing units affiliated through a large and efficient sales organization centred in London. It was a bold and far-seeing conception, sacrificing some cherished doctrines of efficient organization in favour of less palpable, but none the less real, psychological and commerical considerations.

Hirst was amongst the earliest industrialists to realize that science must have a vital place in industry, but that to function effectively science should be focused in its own establishment, having its unique organization and management. In 1919 he founded central Research Laboratories for the use of all the establishments of the Company. These were located at Wembley and grew rapidly as their usefulness was proved, until the staff numbered over 500 in the later years of peace.

When war broke out in 1939 the General Electric Company, with its activities in all parts of the country, was soon seen to be a major national asset, and before his death Lord Hirst had the satisfaction of seeing the creation of his life spring immediately to action in the country's service, a demonstration of virility and a well recognized tribute to sound and strong building over bygone years.

His wide experience and reputation for disinterested counsel caused Lord Hirst's advice to be sought by a wide circle of industrialists and politicians of very varied shades of opinion. In his closing years he was ready to see others carrying on his work—encouraging and helping them, but never interfering or dictating. It is no wonder that he inspired affection and respect.

Lord Hirst joined the Physical Society of London in 1911 and remained a Fellow till his death on 23 January 1943. He valued with humility the welcome accorded to him in scientific circles ; and when he had attained great fame and standing in the country he still held in undiminished respect those men of science on whose work he was one of the earliest to realize that the fate of British industry must depend in the future.

C. C. P.

LEONARD DARWIN

Major Leonard Darwin was born in 1850, being the fourth son of Charles Darwin. He was educated at the Royal Military Academy, Woolwich, and was commissioned in the Royal Engineers in 1871. In 1877 he was appointed Instructor at the School of Military Engineering at Chatham, and it was at this date that he became a life fellow of the Physical Society. He held the post for five years, giving instruction in chemistry. From 1885 to 1890 he was in the Intelligence Division of the War Office, dealing principally with the West African branch, and he then retired from the Army with the rank of Major. He was in Parliament as a Liberal Unionist from 1892 to 1895. From 1893 to 1908 he was Honorary Secretary of the Royal Geographical Society. He became President in 1908 and held the office till 1911. In 1911, on the death of Sir Francis Galton, he succeeded him as President of the Eugenics Society, an office which he held till 1928. He died on 26 March 1943 at the age of 93.

Leonard Darwin had an essentially scientific outlook on the world, but no specialized training in any branch of science, and no special knowledge of physics. His interests were very wide. He was one of the first to use photography in the army, and this went back to the days of wet plate photography. He joined the expeditions of 1874 and 1882 to photograph the transits of Venus in New Zealand and Australia respectively, but both times the sky was cloudy, and the only outcome of the expeditions was certain longitude observations. In 1886 he photographed the total eclipse of the Sun at Grenada in the West Indies. During his Parliamentary period he wrote on economic subjects including bimetallism. In his later years his chief interest was eugenics, and it was largely his sanity of judgement that guided the Eugenic Society in its difficult course. C. G. D.

ARTHUR LAIDLAW SELBY

Professor A. L. Selby died at Llandaff, Glamorgan, on 22 July 1942. He had been Assistant Professor of Physics at the University College, Cardiff, from 1890 to 1897 and Professor from 1897 to 1926, when he became Emeritus Professor.

Selby was born on 2 July 1861 at Atherton, Lancashire. He was educated at Haversham Grammar School and at Queen's College, Oxford. From 1884 to 1890 he was demonstrator in Physics at the Clarendon Laboratory, Oxford, and was a Fellow of Merton from 1886 to 1893. In 1889–90 and 1900–01 he was an examiner in the Honour School of Natural Science. He became a Fellow of the Physical Society in 1885, and was the author of a text-book of mechanics.

Selby carried on his college work year by year with little change and was particularly interested in his Honours Class. Once a week he placed at their disposal an hour of his time when they could bring before him their special difficulties. He insisted on their working hard, and the number who graduated with first-class honours is evidence of his capacity to teach. At the time Selby was appointed, ability to direct research was not considered an indispensable qualification in a professor, and his training under Clifton at Oxford had not stimulated in him a liking for practical work. Although he had no experience of experimental research he realized as time went on that a science department of a university college was not fulfilling its functions if it did not foster original work.

When the present writer, after graduation, obtained the Isaac Roberts Research Scholarship, Selby was very pleased at the opportunity it provided for some research work to be undertaken in his department. The investigation was one into the magnetic properties of some manganese steels provided by the late Prof. A. A. Read, who was working on these steels with Prof. Arnold of Sheffield. The only material available was in the form of short cylinders, so the problem resolved itself to devising a suitable method for dealing with these specimens. Facilities for the construction of special apparatus in the Physics Department of the College were extremely poor, and ultimately the help of a local garage was obtained. There was a scarcity of the commoner types of instruments and, for the measurement of current, resort had to be made to a tangent galvanometer. As a commentary on this state of affairs, it might be remarked that when, some years earlier, Viriamu Jones was Principal at Cardiff and professor of Natural Philosophy, he had been able to obtain the costly apparatus for the determination of the ohm. After much toil and anxiety the investigation on the magnetic steels came to fruition and a paper was published which gave great pleasure to the Professor. This was the first and only research suggested by Selby during his tenure of office.

Selby was most conscientious in attention to detail in connection with his college work. Plans for new physics laboratories in Cathays Park were passed in 1910, but building was not commenced until 15 years later, and for the two years before his retirement Selby was pre-occupied with the problems arising out of the erection of these laboratories. The Viriamu Jones research laboratory had been built about 1911, but the design of this building and the supervision of the work therein was the care of Principal Griffiths.

Dr. J. H. Shaxby, who was lecturer in physics under Selby for many years, has given the writer the following recollections of the Professor : " He was a conservative character and his favourite epithet for any policy or scientific theory he approved of was ' Sound '.

He followed the middle path and was extremely fair-minded and just in his dealings with everybody.

" He was deeply patriotic and during the 1914–18 war took the greatest interest in old students in the Forces. He sent parcels regularly to a number of them, including men who were amazed to get them, knowing they had been on rather bad terms with the Professor of Physics, who had found their college work very unsatisfactory. When ex-service men appeared at the end of the war he was deeply interested in their affairs.

" He was loyal to his staff and had the wisdom to know that the organization of the practical work could be done better by others than himself.

" His chief interest outside Physics was in History, of which he had a very sound knowledge, British history chiefly. He played tennis (with a tricky underhand service), and later golf, but was not a great sports enthusiast. A few hours' quiet walking in the country with two or three friends was a weekly pleasure to him ; he was not a naturalist, but took great pleasure in the countryside and the changing seasons.

" In late years his increasing deafness cut him off a good deal from affairs and he ceased to turn up at classical concerts, at which, after his marriage, he was a pretty regular attendant, though he did not really care much for music ; in fact, it was thought (from lab. experience) that he was a bit tone-deaf.

" His qualities made him a very valuable man on College and University committees of all sorts, where his careful judgement and power of looking at all sides of a problem were invaluable ; some newer members of Senate of course found his conservatism a bit trying on occasion, but they often had to admit in the end that he had been right ".

A fund raised in 1931 by staff and students, past and present, of University College, Cardiff, commemorates his services and their esteem by the Selby Lectures, of which five have been delivered on subjects related to Physics.

Shortly before his retirement Selby married Dorothy Beck, of Great Amwell.

Ezer Griffiths.

ALEXANDER RUSSELL

Alexander Russell was born at Ayr in 1861. He received his early education at Ayr Academy and Glasgow High School. At the age of 16 he proceeded to Glasgow University, graduating M.A. with first-class honours in Mathematics and Natural Philosophy in 1881. A year later he entered Caius College, Cambridge, with an Open Scholarship, and read for the Mathematical Tripos, gaining a place about midway in the list of Wranglers in 1885. After short spells of teaching at Cheltenham College and at the Oxford Military College he joined the staff of Faraday House in 1890. Thus began a long and notable association with this engineering college, only terminated by his death on 14 January last. He was its Principal from 1909 to 1937, serving afterwards in the capacity of Advisory Principal. From 1920 he was also a Governor of the College.

Russell's life in London was also to be closely woven with two scientific bodies: the Institution of Electrical Engineers and the Physical Society. The first of fourteen papers published in the *Journal* of the Institution appeared in 1899. These papers were invariably mathematical in character, and, like his frequent verbal contributions to discussions, served to add precision to the treatment of the various problems occupying the attention of electrical engineers at that time. In only one instance was a paper by him actually read and discussed at a meeting of the Institution. This was in 1907, the subject being " The Dielectric Strength of Insulating Materials and the Grading of Cables ". He was President of the Institution during the session 1922–23. Much later (in 1940) the Institution awarded him the Faraday Medal.

His association with the Physical Society began in 1905. His first paper, " On the Dielectric Strength of Air ", read in that year, struck the keynote of a subject to which he frequently returned. Some thirteen further papers were published in the Society's *Proceedings* in the next twenty years. Again the treatment is mainly mathematical, as is also that of four papers published in the *Proceedings of the Royal Society*. He was particularly interested in developing formulae expressing the intensity of electric fields, and the forces of attraction or repulsion, between two electrified conducting spheres. Much attention was devoted to finding the forms most suited to rapid computation to any desired accuracy of approximation. His work in this field helped to establish what has become a standard method of measuring high potential differences. The advanced student will find very instructive his paper entitled " The Mathematical Theory of the Magnetic Field round a Circular Current, and Allied Problems " (*J. Instn. Elect. Engrs.*, November 1928), which shows how the elliptic integrals K and E make their appearance in the formulae for evaluation of the forces between circular currents, and the mutual inductance between them. In both his presidential addresses, to the Physical Society and the Institution of Electrical Engineers, he gave a résumé of recent trends of discovery in physics. These addresses contain many shrewd observations, though he expressly disclaims any special qualifications for the task and puts his views forward as merely those of " the average scientific man ".

Russell's career is impressive as a striking example of the triumph of character. His imperturbable temperament, genial manner and strong common sense, backed by a fine physique, won for him positions of high honour and responsibility. A usual formula of his in committee was : " I agree with Mr. A.". Couched in even tones, this simple contribution perhaps served to avert disruptive discharge when the atmosphere was becoming electrical. The year 1924 was his *annus mirabilis*. In that year he was the president both of the Physical Society and of the Institution of Electrical Engineers, was elected a fellow of the Royal Society, delivered the oration at the Kelvin centenary celebrations in the University of Glasgow, and received the honorary degree of LL.D. from that University.

He was the author of two well-known standard works : *The Theory of Alternating Currents* (in two volumes) and *The Theory of Electric Cables and Networks*. His small book, *Lord Kelvin : his Life and Work*, contains some entertaining reminiscences of a genius admired and revered, if not always understood, by his pupils. To give an example, Thomson's by no means infrequent digressions on any idea engaging at the moment his creative activities naturally led to an accumulation of arrears in the subject-matter of his lecture courses. To quote Russell : " The last day (of the course) was an eventful one, the professor lecturing and showing experiments to those of the class who could remain, long after the hour was up. The writer was one of those who remained to the end—a period of over four hours—in 1878. . . . . The memory of that last lecture is a treasured possession ".

A portrait of Dr. Russell appears in vol. **36** of the *Proceedings*. D. O.

THE Physical Society and the scientific world in general have suffered a great loss in the death on 14 January 1943 of Dr. Alexander Russell. In the domain of electrotechnics he was an expert mathematician, while in the social world he set a splendid example of modest kindliness and unfailing good nature. He never said a hard word to anyone. I met him first in the 80's, and I can still picture to myself the bonnie tall Scottish laddie who was so friendly and so full of quiet Scotch humour. Faraday House was just beginning its career when he joined the staff in 1890, and in 1909 he became Principal, which position he filled most successfully until he retired in 1937. During this period he wrote many papers for the scientific societies and journals, in particular a series of articles on alternating currents for the *Electrician*. He finally collected these and published them in book form in two volumes.

But it is not his technical work but his social work that made Faraday House what it is. He had many old students all over the world, many of them holding important posts. When on leave they all came to see their old Principal, telling him their experiences and their difficulties, and always getting from him wise and valuable advice for the future. They now cherish his memory with real affection.

In 1890 he married Edith, daughter of H. B. Ince, Q.C., M.P., and has left a daughter and a son, both of them now medical specialists in London.

In his last illness he lay helpless for a year, but through all his trying weakness his brave and noble spirit shone forth until the end came. A. CAMPBELL.

I FIRST met Russell in 1909 when he had just been appointed Principal of Faraday House, and I was a member of his staff from 1911 to 1920. At the time of his appointment the fortunes of Faraday House were at a low ebb, but under his wise guidance and with the advertisement which his growing reputation afforded, the succeeding years saw an almost continuous growth in the numbers of students who were attracted to the College. As a chief he was wise, kind and sympathetic and could always spare time to discuss any matter in which his advice might be sought by younger members of his staff. The style of his teaching can be gathered from his books, which were based largely upon his lecture notes ; he confined himself mainly to mathematics and to the mathematical sections of electro-technology. He was often over the heads of the majority of his students, but those who could follow him did so with profit and delight, and the remainder were not unduly discouraged.

He was appointed to the staff of the College in 1890 at a very early stage in its history and at an important period in the history of electrical engineering. The work of the testing and standardizing laboratories of Faraday House was at that time almost equal in importance to its educational activities, and Russell kept in close touch with the development going on there. In this way, and through his contact with old students, whose close support has always been a strong feature of Faraday House, he kept himself informed of the problems of the industry, his appreciation of which was always rather surprising, considering his own lack of engineering training. His main interest remained mathematics, however, and one seldom entered his room without finding him deep in some mathematical problem. Many of his more popular studies, for instance on the theory of numbers, were published in the *Faraday House Journal*, to which he contributed profusely for many years. Russell was very popular with his old students, by whom he will be greatly missed ; large numbers of them used to call and revive old memories in his genial presence when visiting or passing through London. He had a gift of dry humour which was always a prominent feature at the annual dinner of the Old Students' Association, where he followed, as the official speaker, his old friend, the founder of the College, Robert Hammond.

F. T. CHAPMAN.

ROBERT WILLIAM PAUL

By the death of Robert W. Paul, on 28 March 1943, the Physical Society has lost a friend of many years' standing and the electrical instrument industry one of its pioneers.

Paul was born at Highbury, London, in 1869, and was educated at the City of London School and at the City and Guilds Technical College, Finsbury. He worked in the electrical instrument shop of Messrs. Elliott Brothers and obtained there a practical knowledge of instrument making which was invaluable to him. In 1891 he started business for himself as an instrument maker in Hatton Garden, using his inventive powers in developing electrical laboratory instruments. He worked in close touch with Professors Ayrton and Perry and Mr. (later Professor) T. Mather, who at the time were engaged in designing electrical instruments. In 1900 Paul transferred his works to Muswell Hill, and it was there that the majority of his instruments were made.

In 1903 he invented a moving-coil galvanometer (Patent 6113 : 17 March 1903) in which the coil was mounted on a single pivot " at or about its centre of gravity ". The pivot rested in the centre of a steel ball which was fixed between the poles of a permanent magnet. The coil being supported in this manner, the instrument was not dependent on exact levelling, and possessed a sensitivity far beyond that of any pivoted-coil galvanometer then on the market. The first model gave a full deflection for about 60 microamperes, the coil having a resistance of about 50 ohms. Paul gave the instrument the eminently suitable name of *Unipivot.* Realizing the small amount of energy required to actuate the galvanometer, he developed a series of resistances, shunts, etc., which made it an invaluable instrument for laboratory and test-room work. He fitted an efficient locking device which held the coil with its pivot away from the jewel, thus rendering it safe for transit. He was fond of demonstrating the efficiency of the clamping device by throwing the galvanometer in its leather case downstairs, or even using it as a football.

He was one of the pioneers of the cinematograph, and was the first in Great Britain to make a projector for showing pictures continuously. In an interesting paper, he has told the story of how he became interested in moving pictures.* He describes how, in 1895, he had made about sixty kinetoscopes to an American design, and, when watching the crowds waiting in a queue at an exhibition at Earl's Court to look through some of the machines, considered the possibility of throwing the pictures on a screen.

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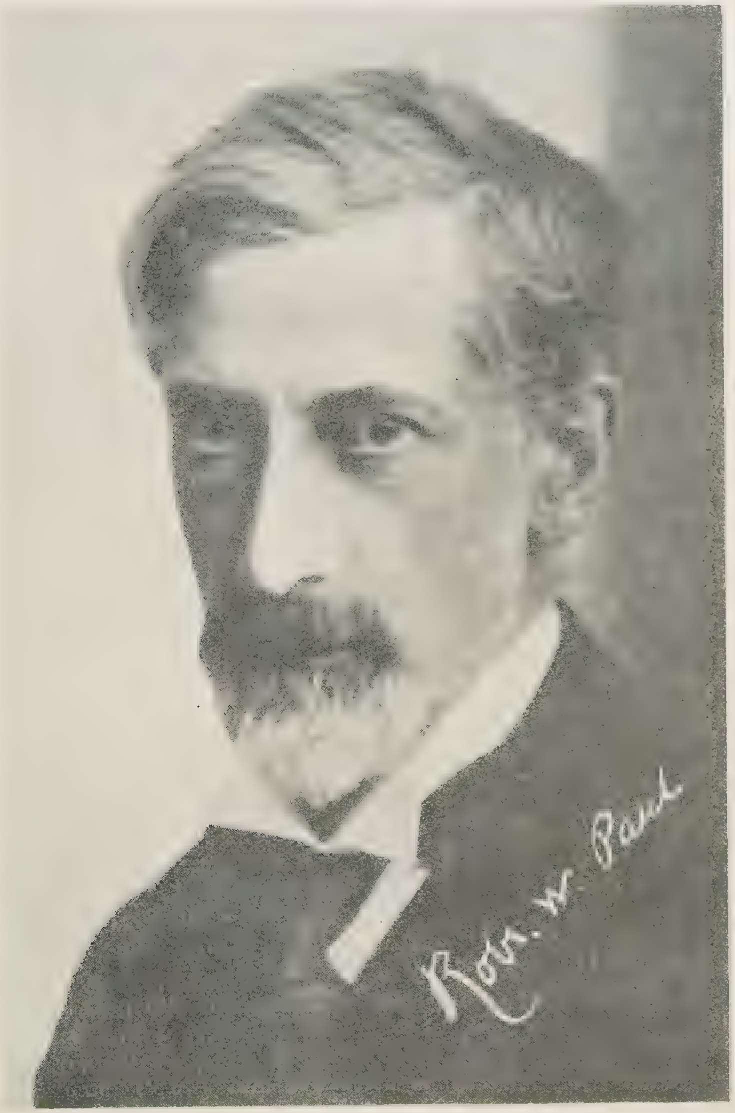
* " Before 1910 : Kinematograph Experiences ", by R. W. Paul, C. M. Hepworth and W. G. Barker. *Proceedings of the British Kinematograph Society*, No. 38, 1936.

In the same paper Paul describes how he adopted a modification of the Geneva stop as used in watches to give an intermittent motion to the sprocket wheel moving the film.



ROBERT WILLIAM PAUL.
Sixteenth Duddell Medallist; founder of the R. W. Paul Instrument Fund.

He soon realized the importance of keeping the film taut whilst being shown, and introduced the duplicate sprocket wheel, the film being stretched between the two wheels, the idea being incorporated in his Patent Specification No. 4686 of 2 March 1896, together with the Maltese Cross mechanism for feeding forward the film intermittently. This mechanism is still largely used.

The first published description * of the projector, or " theatrograph " as it was called, is of sufficient interest to be reproduced here :

> " The theatrograph is a new mechanism for throwing on a screen, so as to be visible to an audience, theatrical scenes or events of interest, with their natural motions and in life-size. The apparatus is simple, and can be adapted to any lantern, and the films containing the instantaneous photographs which produce the effect, and of which there are about 800 to each scene, are of the same size as those used in the kinetoscope. The first public exhibition of this apparatus was to take place at the Finsbury Technical College on the 20th inst. on the occasion of the Annual Conversazione. Mr. Paul, of Hatton Garden, is introducing it ".

The first demonstration actually took place at the College on 20 February 1896, as announced.

News of its success soon spread, and Paul had many invitations to demonstrate his apparatus. The management of the Alhambra Music Hall in Leicester Square engaged him to give an item every evening for two weeks, but, so great was his success, that the engagement was extended to two years.

Paul went to immense trouble to make and take pictures of current events. In June 1896 he photographed the Prince of Wales's horse Persimmon winning the Derby, and projected it himself the following evening at the Alhambra. The enthusiasm of the audience was extreme. It insisted on the film being shown three times over, and it is said that Paul himself was called before the curtain six times.† In 1899 Paul erected a studio at Muswell Hill for the making of cinematograph pictures—the first in Great Britain. The stage was fitted with many of the gadgets which are now the commonplaces of the cinematograph theatre. Many hundreds of films—humorous, dramatic and trick—were produced in the studio. A particularly realistic and successful film of a railway collision was made from models. This must have been one of the earliest attempts at what has now become a high art—the photographic reproduction of important things, or events, by means of models.

Paul was also alive to the possibilities of the cinematograph as applied to scientific work. In conjunction with Professor Silvanus Thompson and a group of students, a series of diagrams illustrating lines of force in changing magnetic fields was drawn ; these diagrams were photographed separately and then projected continuously.

On 9 January 1901, Silvanus Thompson gave a lecture in the Urania Theatre, Berlin, on " Faraday und die Englische Schule der Elektriker ", in which he projected a film made from a series of drawings showing the way in which the lines of force " snap across " from tooth to tooth of an alternator. In October 1911, Paul showed the film to Edison, at his home in West Orange, N.J., and presented him with the original negatives.‡ About 1912 he disposed of his cinematograph rights, and no longer interested himself in the industry.

During the war of 1914–18 Paul used his inventive powers and his great capacity for work in many directions. Dr. C. C. Paterson tells one characteristic story of him in connection with the first production model of the Peterson-Walsh auto-aircraft height-finder which he had made. It had been arranged that at three o'clock one Saturday afternoon, in 1915, a demonstration of all the various height-finders which were competitors for official adoption should be given on a field at the National Physical Laboratory. Dr. Paterson writes :

> " All the height-finders, except Paul's, were erected on the field and got into working order by their sponsors in the morning ; but Paul's failed to turn up, and when two o'clock came and the Brass Hats began to assemble, it seemed clear that something must have gone wrong.

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* *English Mechanic and World of Science*, vol. **63**, p. 11, 21 February 1896. (A further description, with an illustration of the apparatus, was published on 6 March in the same volume.)

† The *Strand Magazine* (vol. xii, p. 134, 1896), under the title " The Prince's Derby, Shown by Lightning Photography ", published a dramatic account of Paul's adventures in photographing this Derby. In it an illustration is given of him and his camera ; in the text it is stated : " Mr. Paul is seen with his unique camera ; he is looking into the ' finder ', ready to commence turning the hand-wheel the moment the desired picture came into the field ". The article contained reproductions of several photographs " selected from Mr. Paul's set of 1,280 ".

‡ See *The History of the Institution of Electrical Engineers*, by Rollo Appleyard, 1939, p. 182.

> "At a quarter-to-three a cloud of dust appeared, and with it Paul's disreputable Gladiator car, driven by himself and with half-a-ton of height-finder on the back—depressing the back springs to the axle. The impact of the car on the boundary to the demonstration field caused it to bounce into the air and to continue to bounce and creak in a hair-raising way as it risked its heavy load to the erection site. Here, with no particular care, the height-finder was dragged down and by three o'clock it was set up and ready for trial.
>
> "There was more than a suggestion of remonstrance at taking such risks on a critical occasion, but Paul's only response was that if the thing had to be handled in practice by Army personnel in the field it was just as well to test it for reliability as well as for accuracy. The gear was found to be in good adjustment, and this, combined with Paul's demonstration and supreme confidence in his own design, resulted in the adoption of this height-finder for use at Field A.A. stations in the last War".

In 1920 his business was amalgamated with the Cambridge Scientific Instrument Company, the combined firm then being known as the Cambridge and Paul Instrument Co., and now as the Cambridge Instrument Company, Ltd. During recent years he developed, with Sir William Bragg, the Bragg-Paul pulsator, an apparatus for assisting breathing in cases of respiratory paralysis. He had the satisfaction of knowing that this apparatus had been instrumental in saving the lives of several children.

The scientific instrument-making industry owes much to Paul's efforts to improve the technical education given to its workers. He advocated, and, indeed, put into practice in his own works, the holding of technical and theoretical classes for the training of apprentices in the works. It was due to his initiative that the Physical Society inaugurated the annual competition for apprentices and learners at its Annual Exhibition of Scientific Instruments and Apparatus. He showed his further interest by founding the Paul Scholarship, which is administered by the Institution of Electrical Engineers, for enabling young students to enter the works of a firm of scientific instrument makers for two years' training.

Paul much interested himself in the work of the Physical Society, serving as a member of the Council from 1923 to 1928, and as Honorary Treasurer from 1935–39. He also served as a Vice-President from 1928–1931, and again from 1939–42. It may now be stated that he was the anonymous and generous donor who gave £700 to the Society to assist it through the difficult time experienced early in 1940. He was awarded the Sixteenth Duddell Medal in 1938. He was a Founder Fellow of the Institute of Physics and served on its Board from 1923 to 1931, being a Vice-President for the last four of these years. He had also served on the Council of the Institution of Electrical Engineers and as a Manager and Vice-President of the Royal Institution.

Mention should be made of the admirable collection of electrical instruments that he assembled at the Royal Albert Hall in 1931 for the Faraday Centenary Exhibition. The part of the catalogue describing these forms a useful handbook on the subject.

Under his will, Paul left industrial shares valued at over £100,000 to form a Trust to be known as "The R. W. Paul Instrument Fund". The income from the Fund is to be used primarily for the provision of instruments of a novel or unusual character to assist physical research. In particular, Paul wished to help those whose researches were impeded for want of an instrument of a new type and for which the usual funds of a University or other Institution were not available owing to the expense involved or for some other reason. The fund will be administered by trustees acting in conjunction with a committee the members of which will be appointed by the Royal Society, the Physical Society, the Institute of Physics and the Institution of Electrical Engineers.

Paul combined immense energy with initiative and inventiveness. He possessed shrewd business acumen, and, with it all, a dry sense of humour. His was a modest and generous disposition which brought him many friends. R. S. W.

ROLLO APPLEYARD

The late Commander Appleyard first came into my life more than fifty years ago at the offices of the *Electrical Review*, where I was a sub-editor of nineteen under that remarkable chief the late T. E. Gatehouse. Born on 9 January 1867, Rollo, as we always spoke of and addressed him, was my senior by some six years, a difference which means much when you are young, but vanishes as time passes. He had followed his education at Dulwich Old College by a period in the laboratory of Ayrton and Perry, where important pioneering work was done in connection with electrical and other scientific instruments, and by college demonstration in physics and telegraphy ; subsequent experience in practical engineering work led on to service on the staff of the Royal Indian Engineering College at Coopers Hill. There he remained from 1885 to 1892, gaining knowledge which qualified him for writing critically regarding developments.

He entered the service of the India Rubber, Gutta Percha and Telegraph Works at Silvertown at the age of 25 and remained with that concern until the outbreak of war in 1914. At about this time he made the acquaintance of my chief at musical evenings held in South London, and possibly they met at Silvertown, where Gatehouse, still practising as a consulting engineer, frequently tested submarine cables. In this way began that literary association which Appleyard told me did much to influence his tastes and fit him for an almost unique position in scientific and critical technical journalism. That the editor did not err in his judgement of the capabilities of his young protégé, when he invited him to contribute to his journal, is fully borne out by the enviable technical literary record extending over the whole of the fifty years which have elapsed since I was privileged to deal with some of his first manuscripts. From our early days together until the last, Appleyard was to me a man of great personal charm and cultured courtesy and a contributor to whom the editorial room was always open. What he wrote was always characterized by that same charm and courtesy, however critical it had to be ; his " copy " was exceptionally clear and his proofs were remarkably clean—so unlike the handwriting of many celebrated men of science and so free was he from the all-too-common passion for corrections, involving sometimes the re-setting of an entire article. Some of his contributions might well be re-published to-day as examples to be followed by young aspirants to scientific literary fame ; notably I recall one of 1897 on " Twenty-five years of Experiment in Physics ", and another on his special subject, " Submarine Telegraphy ", in 1892. The former article he characteristically opened thus :—

> " Ask not the latest news of the last miracle . . . .
> Ask what most helps when known, thou son of Anchitus ".

Then there was his useful service to the Press when he was officially reporting the *Proceedings* of the Physical Society ; his literary work and advice for *The Times* Engineering Supplement while he was still at Silvertown ; his work in connection with the Faraday centenary celebrations ; his history of the I.E.E. ; his share in *Science Abstracts* ; his letters to *The Times*, and much more that space does not permit me to detail.

I have mentioned the " accident " of friendship which, in my judgement and on his own acknowledgment, started Rollo Appleyard on his true career. I am sensitive as to whether I ought to reveal another experience which may have helped to alter the course of things for him—one of those little things from which great issues sometimes spring. Immediately after he joined the Silvertown Company in 1892 he was sent on a submarine cable expedition to Brazil, but on his return it was whispered that nature's tossing in a cable-ship had been such that he was unable to concentrate his thoughts as adequately upon cable-laying duties as he desired, and such expeditions were not to fall his way thereafter. To travel in the " Terrible " as a *Times* correspondent in the Naval manoeuvres some 14 years later was a different matter. One's thoughts turn irresistibly to the story that another eminent scientist of the same period could not contemplate a trip on the Thames from the Old Swan Pier to Battersea without experiencing an internal paddle-wheel effect !

Much of interest might be written of the 22 years spent with the Grays at Silvertown, chiefly in submarine-cable design and production ; but I can personally vouch for the pride and satisfaction with which he told how it was on his own initiative, and only after great efforts to convince superiors, that a vast improvement could be introduced into the dielectric of the core of the great Pacific Cable between San Francisco and Honolulu. Reviewing his career as a whole one is reminded again of the inequalities with which men of eminence in different fields are rewarded. Men who were starting their careers at about the time I first met him have reaped vast financial reward and been awarded the highest honours by the State, but when I saw the announcements of two recent wills, and one of them was Rollo's with a modest £2000 to £3000, the words came quickly to my mind:

> " A man's life consisteth not in the abundance of the things that he possesseth ".

Rollo Appleyard rendered sterling service to science and to literature, also to naval and engineering life, and he was much loved by his fellows, but others who chose the productive industry side of science, or finance, passed him by in those things which this world counts success. Yet science was indispensable to their fortunes. She was still the Cinderella of our Household!

One episode more. When the 1914 war broke out, Rollo Appleyard left Silvertown and went to the Admiralty, where he remained on special and varying duties until 1920. Uniformed officials from Whitehall followed one upon the heels of another to my office on Ludgate Hill asking whether we of the *Review* could furnish them from our extensive library of technical works with pictures of factories, power stations, etc., on the Western Front which should be bombed as soon as our planes could reach them. I immediately concentrated upon this work, thinking it important and urgent. A table was set apart upon which numerous book-marked volumes were held in readiness for the promised collection. Some six months elapsed, and no demand was made for the information so eagerly sought at the beginning. Then, one day, who should arrive but Rollo on a similar mission. He was lent to the Air Service in 1917 but was unaware that anybody had previously asked for our help. He soon grasped the situation, and nobody will be surprised to hear that within almost less time than it takes to write he had called a taxi, packing cases had been filled and we were on our way ; even then there was no assistance available at Whitehall, and we had together to take those heavy cases along corridors and up flights of stone stairs to deposit them in a special room. But it was with a sigh of relief that I saw them there. The volumes came back in due course with thanks for the loan and the added compliment of further inquiries regarding specified targets for bombing. This episode remains in mind as only one of several surprise visits in which Rollo came to my relief. He came to see me when he was writing his *History of the Institution of Electrical Engineers* to check up some dates and facts from my records, but the last occasion of our meeting was in 1938 when I was in a North London Nursing Home, after two major operations from which it was doubtful whether I was going to recover. It was an unannounced visit and helped to bring me back again to outlast him. He died on 1 March 1943. Amongst all the faces that come before me as I look back over these fifty years—great engineers, eminent scientists, brilliant newspaper proprietors and editors—none is recalled with greater affection and satisfaction than that of Rollo Appleyard. May there be many more like him to sweeten life in the coming days.

ALBERT H. BRIDGE.

THOMAS BERNARD VINYCOMB

T. B. VINYCOMB was born on 2 November 1878, the second son of the late John Vinycomb, M.R.I.A., of Belfast. He was educated at the Royal University of Ireland, where he graduated B.A. in Physics with First Class Honours, M.A. in Mathematics with First Class Honours, and obtained a Fellowship in Physics. Later he was a post-graduate student at the Cavendish Laboratory, and a Member of the Royal Irish Academy.

In 1907 he was appointed Head of the Physics Department of the Woolwich Polytechnic, and there he remained until he retired through ill-health in 1942. He died in July 1943.

He served with distinction in the Great War from 1916 to 1919, won the Military Cross, and was posted to the Regular Reserve of Officers with the rank of Captain.

In the University of London he was a recognized teacher of physics and of electrical communications, a member of the Faculty of Science and of the Board of Studies in physics, and on many occasions examiner both in physics and in communications.

He was a student to the end. Always eager to keep abreast of the latest developments in his subject, he was always well informed and most thorough in assimilating new publications. He read everything with ease. He was also a great teacher who used methods which were both original and effective. He really studied teaching and took the greatest pains to help his students. His model at the Science Museum illustrating wave motion is a permanent record of his inventive skill, and his contribution to *Technical Electricity* made that textbook possible.

Strangely, perhaps, in view of his learning and professional qualities, including something approaching absent-mindedness, he was a most effective administrator, and ran his department well. The development and expansion of the Woolwich Polytechnic was helped on by him in no small measure. He was an inspiration to colleagues and students alike. He neglected his health for his work, in spite of the efforts of his always charming and devoted wife. The writer remembers an occasion some years ago when a telephone call from Mrs. T. B. announced that T. B. was ill in bed and would not be at the Polytechnic for a few days, followed within an hour by T. B.'s entry, announcing " I've escaped ".

The world is the better for his having lived. He was a great student, a great teacher, and a great gentleman, and will be sadly missed.

E. MALLETT.

HAROLD DENNIS TAYLOR

FOLLOWING a family tradition, Dennis Taylor, who was born in 1862, was educated at St. Peter's School, York. He was in process of training to become an architect when, as a result of his scientific inclinations, an opportunity was offered him of joining the old York firm of T. Cooke and Sons. Shortly after taking up this appointment, he, in 1885, took out a patent—first of some fifty—for a photographic exposure meter. This was followed by a new type of astronomical objective, and this again by the " Cooke " Photographic Lens and a variety of others relating to all classes of optical instruments.

Many of these inventions were successful and a few even of outstanding importance, such as the " Photo-visual " objective patented in 1892, the first of its type to be produced commercially anywhere in the world, and the " Cooke " Photographic Lens, which, following a long period of stagnation in lens design, opened up a new line of development which had been entirely overlooked and led to most important developments which still continue. So excellent were the results obtained that lenses up to an aperture of 12 inches were made. They were used by astronomers to photograph the whole of the northern and southern hemispheres. Certain other patents proved to be remarkable anticipations ; for instance, he discovered in 1904 a process by means of which the light transmission of optical glass could be increased, and in the last few years this has been done on a very large scale ; similarly, in 1907, he was the first to adopt the built-up " Optical Square " in range-finders, a practice which is now quite generally adopted, on the Continent as well as elsewhere.

In 1893 Dennis Taylor was appointed Optical Manager to Messrs. T. Cooke and Sons, and this was followed in 1895 by a seat on the Board of Directors. For the next twenty years he devoted himself to the improvement of optical processes and the development of new machinery and testing equipment. He was the first to use the star collimation for the testing of the performance of telescope objectives, and this form of test is still widely used today.

About 1895 a visit was paid to the well-known German glass works of Herren Schott und Gen. in order to ensure the supply of certain optical glasses then made only in Germany, and there he met Professor Ernst Abbe, who did so much to develop the German optical industry.

In 1891 Dennis Taylor wrote his first book, *The Adjustment and Testing of Telescopic Objectives*, and this was followed in 1906 by the publication of *A System of Applied Optics*, in which he developed a complete system of formulae for the use of those engaged in optical design. These led, in his hands, to astonishing results, for it was due to this investigation that he discovered the theoretical principle later applied to the construction of the Cooke Lens. He also contributed valuable articles on optical matters to the *Monthly Notices* of the Royal Astronomical Society, to the Optical and other learned Societies and to the *Encyclopaedia Britannica.*

In 1926 Dennis Taylor delivered the 26th Traill Taylor Memorial lecture to the Royal Photographic Society, in 1933 he was presented with the medal of that Society, for contributions to the development of photographic lenses, and in 1934 he received the Duddell

Medal of the Physical Society for contributions to the advancement of knowledge by the invention and design of scientific instruments.*

A man of wide interests and encyclopaedic knowledge, Dennis Taylor had many hobbies, including gardening, photography, astronomy and natural history. Of simple tastes and inquiring mind, he loved the outdoors and the quiet beauty of the countryside.

Following his retirement from active business, Dennis Taylor elected to live in the charming village of Coxwold, Yorks, amid a peaceful scene that he and his ancestors before him had learned to love. He passed away peacefully on the night of 26 February 1943 after a short illness, and leaves a widow, two sons and a daughter.

Dennis Taylor's writings are of special interest to those members of the Physical Society who work in optics. To those who know them, whether books, pamphlets or patent specifications, no word of commendation is needed. Both in this country and abroad—particularly in the United States of America—they are recognized as masterly expositions of their subjects, and are to this day valued as guides and reference works. Unfortunately they are not all easy to obtain ; the *System of Applied Optics*, for instance, is out of print, so that some of the younger generation of lens designers may not have had an opportunity of studying it, though they may be familiar with the valuable little book on the *Adjustment and Testing of Telescopic Objectives*. It will perhaps therefore not be inappropriate to say a little about the *Applied Optics*, which I think is perhaps the most important of all Taylor's published work. It has served as a textbook and guide to some of our most successful lens designers ; and my own interest in optics, which was not in the least excited by the books read at Cambridge, excellent though they were in many ways, dates from the time I read this book.

There are at least four methods by which designs of optical instruments may be evolved. The first is the method of the workshop artist, who aims at something that his previous experience leads him to think should be near what he wants, and from time to time examines the effect of his work and modifies his programme to keep on the right road. For such workers there is often no paper design of the lens itself. Probably all the early telescopes were made in this way, and Thomas Cooke, the founder of Thomas Cooke and Sons, the firm with which Dennis Taylor was so long associated, was a good example of the artistic worker.† The other methods all make use of theoretical calculations. The second, in which the design is evolved from an algebraic theory, was adopted by Coddington, whose *Treatise on the Reflexion and Refraction of Light* was, despite serious defects, long the leading book on lens design in this country. The third method relies on tracing a number of rays trigonometrically through a system to determine its properties. This method is used very extensively in the optical industry today. The fourth method makes use of Hamilton's Characteristic Function, and appears to have been used little, if at all, in lens design. In theory, this method is more powerful than the others, and has been employed successfully to establish some important results of wide generality ; but it has been generally considered unsuitable for detailed work. In fact, in a period of nearly a hundred years no one had found out how it could be applied to the everyday work of the lens designer. Needless to say Dennis Taylor did not use this method ; it is conceivable that he had not heard of it at the time he was developing his own theory, and it seems safe to assume that he gave it no consideration.

In his theoretical work, Dennis Taylor belonged to Coddington's school, and his *Applied Optics* is essentially an extension of Coddington's work. In developing his instruments, however, he made use of the first method also. No doubt in his early days with the firm this was the method employed, and Taylor would have acquired extensive knowledge of its use (of which he continued later to avail himself) long before his theoretical work was far advanced. But it is clear from his own account that in the design of his lenses he was guided by the formulae he had developed. It will be of interest to quote one or two passages. Following an illustrative calculation on a large telescope object glass, he says :

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* See *Proc. Phys. Soc.* **46** (1934).

† Nevertheless, I learn from Mr. Wilfred Taylor (son of Dennis Taylor and joint Managing Director of Messrs. Cooke, Troughton and Simms) that a letter written by Thomas Cooke about 1860 shows that he used Coddington's Spherical Aberration formula and a trigonometrical trace as a check.

> " Many optical designers would prefer to employ trigonometric calculations of spherical aberration rather than any other, even in the case we have just dealt with. Indeed, it is doubtful whether in the case of some of the highly complex constructions of five or more thick lenses forming modern microscope objectives, any method can be as easily applied as the trigonometrical one, provided that not only the focus for the extreme edge rays relatively to the ultimate centre rays is calculated, but also the focus for the rays passing the aperture at a height y equal to about three-fourths of the full semi-aperture. Thus any discrepancy between the focus for the intermediate zone of the rays and the joint focus for the central and edge rays would at once indicate the presence of an aberration of the order y^4 and perhaps y^6. Or, supposing the focus for the edge rays not to coincide with the focus for ultimate centre rays as calculated by the formulae of the first approximation, then the calculated relative position of the focus for the zone of radius three-fourths would at once show any departure from the law of the aberration varying as y^2 simply, and thus reveal the presence of an aberration of the next higher order.
>
> " It is certainly true that the trigonometrical method is very much more applicable to broad axial pencils than to any other case of refraction that can arise.
>
> " Although trigonometrical calculations of the course of a ray through an optical system are often highly desirable, yet these are merely mechanical processes which, more especially when applied to oblique and eccentric pencils, do not lend themselves at all to analysis. They are empirical and uninstructive, or at any rate barren of enlightenment unless a large number of calculations are carried out in which certain factors, such as radii or separations, are varied, and the results of such variations carefully noted. All this involves much empirical work ; whereas by the aid of algebraic formulae, although they may be not quite so exact, leading principles can be established, and the *tendencies* of the corrections consequent upon the variation of any one term can always be worked out with very little trouble, and it is by the intelligent grasp of the general tendencies that an optical construction may be varied in its parts until the utmost possible perfection is realised ".

He presents his mathematical work fully, and realises the dangers of treating the aberrations separately. For instance :

> " As regards the correction for obliquity we have again arrived at the same result as did Coddington, only we have in Formula IV added the spherical aberration which is common to *all* the pencils, whether direct or oblique. We have recapitulated these processes chiefly in order to form an introduction to more important results yet to be arrived at, also bearing in mind the principle that complex investigations of this sort are understood in less time and with less effort when all processes (except perhaps reductions) are given in full ".

Of special interest are two passages which obviously bear on the development of the Cooke Photographic Lens. They run :

> " So far, then, we have considered the application of the formulae arrived at to combinations of very thin lenses in contact. We have yet to consider their application to either thin lenses more or less widely separated, or to thick lenses considered either singly or in combination. Some twelve years ago, in the course of thinking over the general results arrived at in the last two Sections, especially in relation to the normal curvatures of image characteristic of simple or achromatic lenses, it suddenly occurred to the author that since the normal curvatures of image due to any lens, whether simple or compound, are fixed by its refractive indices and power alone, and are independent of the state of the rays entering the lens, whether convergent, divergent, or parallel, then it should follow that the normal curvature errors of an achromatic and aberration-free collective lens should be neutralised by the normal curvature errors of an achromatic and aberration-free dispersive lens of the same power (and made of the same glasses) placed at a considerable distance behind the collective lens ; while the combination would, *as a result of the separation*, have considerable power or yield a positive focus, so long as the rays from the collective lens are convergent to a distance behind the

> dispersive lens less than the principal focal length of the latter, or more especially when the rays entering the first or collective lens are parallel. But such complete neutralisation of normal curvature errors could obviously not ensue if any E.C.s* were allowed to interfere, therefore both these achromatic and aberration-free lenses must be free from coma or give symmetrical oblique refraction ; otherwise pencils of rays traversing one of the lenses centrally, but the other necessarily eccentrically, would be subject to E.C.s, and their final foci be either shortened or extended, and thus the desired result be prevented. This idea led to further experiments and calculations. . . . "

And

> " Thus the effect of a departure from the Petzval condition is reduced to a vanishing quantity, so that if we construct photographic lenses on the principle of gaining a considerable proportion or all of their power by separation, then we need no longer be restricted to carrying out the Petzval condition ; we can ignore it to some extent in favour of a more general and elastic rule, viz. that the power of the dispersive lens must be approximately equal to the power of the collective lens, or the sum of the powers of the collective lenses if there are more than one.
>
> " This is one of the two supplementary principles which underlie the Cooke photographic lenses, and many others which have been introduced since they were first made public."

In judging this book it is important to bear its date (1906) in mind. For a period of nearly a hundred years no first-class work on applied optics had been published in English. Academic optics lagged far behind knowledge in the optical industry, and the appearance of this book, which bears unmistakable signs of deep study and unstinted labour, went far to remove a defect which ought not to have existed. It is perhaps significant that it was written by a man without University training. In some respects the book belongs to a period that has passed. The industrial methods of today differ from Taylor's, and they, too, may be passing. The theories of the future may be algebraic rather than trigonometrical, but they will not be founded on the direct appeal to geometry, with the punctilious attention to detail, which is characteristic of Taylor's book. We now know how to meet the difficulties which for so long have impeded the use of the Characteristic Function, and this may well be found to be the simplest of all methods for designing new instruments. If this is so, Dennis Taylor's book is likely to remain of special interest to future generations, as representing the highest stage to which optical theory was advanced by the methods he used, just as the Cooke lens will retain its interest by reason of the beauty of its design.

T. SMITH.

FRANCIS JOHN WELSH WHIPPLE

We record with great regret the death of Dr. F. J. W. Whipple, which took place at his home in London on 26 September ; though he had been in indifferent health for some time, the end, which was due to heart failure, was sudden and unexpected.

Whipple was born in London on 17 March 1876 and was educated at Merchant Taylors' School. After a brilliant school career, in which he carried off many prizes and scholarships, he proceeded to Trinity College, Cambridge, with a Senior Mathematical Scholarship. He graduated as Second Wrangler in 1897, and followed that up with a First Class (Division 1) in Part II of the Mathematical Tripos in the following year.

On leaving Cambridge he accepted a mathematical mastership at his old school and remained there until 1912, when he was invited by Sir Napier Shaw to join the staff of the Meteorological Office as Superintendent of Instruments. In that capacity it fell to his lot to arrange the supply of meteorological instruments to the Navy upon the outbreak of hostilities in 1914. This was no easy task, for the Office was ill prepared to cope with the sudden and vastly increased demands which the crisis made on it. Subsequently he transferred from the Instruments to the Statistical Division, into which the British Rainfall Organization was merged after the death of its Superintendent, Carle Salter. In 1925 he succeeded the late Dr. Chree as Superintendent of Kew Observatory, which had come under the direct control of the Meteorological Office a few years earlier. In that post he remained up to the time of his retirement in 1939. There was a peculiar fitness in this appointment, for not only had Whipple's father, G. M. Whipple, been Superintendent from 1876 to 1893, but his grandfather on his mother's side, Robert Beckley, had served there in Balfour Stewart's time and had taken an active part in designing the standard recording instruments of the Observatory. These were the first of their kind, and served as models for the equipment of many other observatories. The original Beckley rain-gauge is still in regular use at Kew after well over 70 years of continuous service. For three generations of one family to be thus intimately associated with a great scientific institution must surely be unique.

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* i.e., eccentricity corrections.

As is evident from his academic record, Whipple was a mathematician of exceptional ability, and his early scientific work was naturally in the mathematical field. His first published paper, " On the Stability of the Motion of a Bicycle ", appeared in the *Quarterly Journal of Pure and Applied Mathematics* in 1899. As a schoolmaster he took his full share in the school life, among other things as an officer in the school cadet corps, but nevertheless he managed to find time and energy for work of his own, and published a number of papers in the *Quarterly Journal* and in the *Journal* of the London Mathematical Society. After his retirement from the scholastic profession, a little time was required to become familiar with the new environment, and, in addition, the outbreak of world war number one brought with it a heavy burden of administrative work. Nevertheless, we soon find Whipple bringing his mathematical tools to work on the problems that were then interesting meteorologists. Papers in the *Philosophical Magazine*, " The Motion of a Particle on the Surface of a smooth rotating Sphere " (1917) and " The Disturbance of the uniform Temperature of the Stratosphere by the vertical displacement associated with horizontal motion governed by the Geostrophic Law " (1923), and one in the *Quarterly Journal of the Meteorological Society* on " The Laws of Approach to the Geostrophic Wind " (1920), mark this phase in his development. About the same time (1920) we find a paper in the *Proceedings of the Royal Society* on quite a different subject, " Equal Parallel Cylindrical Conductors in Electrical Problems ". There followed a return to his earlier interests, and during 1924 to 1926 the two mathematical journals contain numerous contributions from his pen, mainly on hypergeometric series.

After taking over at Kew, Whipple's work and interests entered upon a new phase. The routine of the observatory ranges over a wide field. Atmospheric electricity, seismology, the special study of radiation and, until recently, terrestrial magnetism are included in its activities as well as the more conventional meteorological work. Thus the superintendent is brought into contact with the various bodies, national and international, which interest themselves in the co-operative study of these subjects. Whipple became a member of the National Committee for Geodesy and Geophysics and attended several meetings of the International Union as a representative of the Meteorological Office, while the outbreak of the present hostilities found him in Washington as one of the British delegates to the 1939 Conference. He also served for some time as Chairman of the Seismological Committee of the British Association.

Thus the mathematician became a geophysicist. Whipple read widely and acquired a remarkable familiarity with the literature of the many branches of geophysics that came before him. It naturally came about that he was often called upon to provide a review from the meteorological aspect of " the present state of our knowledge " of some special subject to be brought forward for discussion at a scientific meeting. His many contributions to *Nature* also bear witness to the catholicity of his interests. Circumstances changed at Kew as the scientific staff increased in number. During his long tenure of the superintendentship, Chree had worked alone with only manipulative and clerical assistance, and so it followed that the output of the observatory reached the outer world in memoirs bearing Chree's authorship and usually appearing in the *Transactions* or *Proceedings* of the Royal Society. Nowadays the *Geophysical Memoirs* and *Professional Notes* of the Meteorological Office are the normal channels for publication.

Among Whipple's personal contributions of this nature may be mentioned the memoir entitled " Point Discharge in the electric field of the Earth : an analysis of continuous records obtained at Kew Observatory ", but behind the scenes he took a hand in shaping many others in which younger members of the staff set out the results of the work which had been entrusted to them. A more self-seeking head might have stepped in at the final stages and claimed the credit, but that was not Whipple's way.

Two subjects which lie somewhat outside the normal routine of observatory work attracted Whipple's special attention. One was meteorological optics, on which he became a recognized authority. The article on the subject in the *Dictionary of Applied Physics* is from his pen. The other was the propagation of sound through the atmosphere over great distances. This subject came to the fore during the last war, and after the cessation of hostilities a number of experimental explosions on a heavy scale were arranged on the Continent to collect exact data. In this country the audibility of gunfire was investigated with the aid of recording microphones by the Meteorological Office with the co-operation of the War Office. Whipple was in charge of the work for the Office. He reviewed the whole subject in the Symons Memorial Lecture delivered to the Royal Meteorological Society in March 1935, which is on record in the Society's *Journal*, and he also published numerous progress reports. An aspect of the investigation that particularly interested him was the evidence which it affords of the existence of high temperatures above normal stratosphere levels. In his hands the investigation became, in fact, a means for exploring temperature conditions at such levels.

Life in London affords opportunities for participation in the activities of learned societies of which Whipple gladly availed himself. He joined the Physical Society in 1915 and served on the Council from 1916 to 1921. Even more close was his association with the Royal Meterological Society, of which he became a Fellow on taking up meteorology as a profession. He served on the Council for many years and was its President in 1937 and 1938, and its representative on the Board of the Institute of Physics, of which he was an original member, from 1930 to 1934. He also acted as editor of the Society's *Journal* for a short time, but was compelled to relinquish the task towards the end of last year through ill-health. His absence from the meetings of both societies will be deplored by his many scientific friends. He was a ready speaker and delighted to take part in the discussions, in which he could bring his wide knowledge to bear on most subjects as they came up. He was generous in his appreciation of the efforts of younger men, and his criticisms, when he felt that criticism was called for, were always helpful and never of the wounding type. His innate modesty and sensitiveness prevented him from adopting an overbearing attitude. R. G. K. L.

PIETER ZEEMAN

Zeeman, who was born on 25 May 1865 at Zonnemaire in Zeeland, was the son of a clergyman. After his school days, during which his taste for science was already evident, he proceeded to Leyden University, where he studied under Kammerlingh Onnes and H. A. Lorentz. He remained there, first as assistant and then as privatdozent, until 1897, taking his doctor's degree in 1893 with a thesis on the Kerr effect.

It was in 1896 that he discovered the Zeeman effect, the splitting of spectral lines in a magnetic field. Interesting as the effect was in itself, and brilliant as was the experimental work by which it was found, its main importance lay in the fact that from it could be deduced the existence of electrons in atoms, a discovery which was also being made at the same time in other ways. From the Zeeman effect, very accurate values of e/m were at once obtained, and showed that, if electrons were responsible at all for the effect, they must be vastly smaller than the atom itself.

A year after this work, Zeeman moved to Amsterdam, at which University he was promoted to the post of Professor of Experimental Physics in 1900. His interests remained in the field which he had opened up, and in 1923 a new spectroscopic laboratory was built for him.

His work was widely recognized, and, among other honours, he received a Nobel prize for physics, a distinction also bestowed on another Netherlander, his old teacher Lorentz. His own country recognized his achievements by making him a Knight of the Order of the Netherlands Lion and a Commander of the Order of Orange Nassau. Zeeman was elected a Foreign Member of The Royal Society in 1921 and an Honorary Fellow of The Physical Society in 1929.

He died after a short illness on 9 October 1943, at the age of 78, and is buried at the Kleverlaan Cemetery in Haarlem. With him passes the last of a group of physicists from the relatively small country of Holland who, a generation ago, played such a great part in the foundation of the " new physics " : Lorentz, Onnes and Zeeman. That the tradition of their country is still carried on is witnessed by the name of another Honorary Fellow of our Society, P. Debye, who, like them, will be honoured by physicists throughout the world for many generations to come.

REVIEWS OF BOOKS

The Planning of Science. Pp. 127. (London: The Association of Scientific Workers, 1943.) 2*s.* 6*d.*

The conference on *The Planning of Science*, held by the Association of Scientific Workers in January of this year, will be remembered for a long time. The large hall was crowded, and hundreds of scientists were unable to find a place. The efficiency of the proceedings was equal to the enthusiasm of the audience, and was a demonstration of competence among the new generation of scientists, which is an encouraging sign for the future. The report is packed with interesting topics. It is pointed out that the methods of operational research which are effecting important changes in warfare can also be applied, in civil life, to the design of instruments and machines in any situation and the best way of using them. How far is this attitude from that related of the first flying officers by Sir Philip Joubert: " There was a time when it was believed that only a horseman could fly an aeroplane and our field officers insisted on wearing spurs. We passed successfully through this nursery stage, however, and the riding crop gave way to the spanner, and the text-book on horse-management to the treatise on the internal combustion engine."

A similar change in many other directions is needed to ensure a rational conduct of modern technical society. Sir Lawrence Bragg remarked that the greatest problem confronting scientists is the general ignorance of science amongst politicians, civil servants and leaders of industry, who are inclined to consult the scientist as an expert when in trouble, but not as a colleague in making plans for the future. They appoint advisory committees consisting of the " President of this and the Chairman of that ", who are not necessarily the vital experts who should deal with the job in hand. Sir Lawrence is in favour of more academic scientists with part-time executive positions in Government Research. Such men would be free from administrative trammels and yet have some power. Professor Blackett and others stressed the need for mobility between appointments. It should be possible for a scientist to move between academic, government and industrial research without any difficulty.

The physicist will find in the report many discussions which bear closely on the professional problems with which he is concerned today, and will be still more concerned in the future.

J. G. CROWTHER.

Time Bases (Scanning Generators): their design and development, with notes on the Cathode Ray tube, by O. S. Puckle. Pp. xii + 204. (London: Chapman and Hall, Ltd., 1943.) 16*s.*

The subject of time bases is one on which Mr. Puckle is an acknowledged authority, and his book on the subject will be welcome both to those directly engaged on cathode-ray oscillography and to the readers whose interests are more general. The author, in the preface, states that the book is intended to cover all the more important electronic devices which are available for producing the time bases in television receivers, cathode-ray oscillographs, engine indicators and similar apparatus involving precise timing or measurement of time intervals.

After a brief introductory chapter on the measurement of time, the second and third chapters deal with time-base forms and types of time-base circuits respectively, and bring out in an interesting way the historical development of the subject. Successive chapters then describe trigger circuits ; blocking oscillators and inductive time bases ; polar co-ordinate, multiple and velocity modulation time bases ; linearization of the trace ; push-pull deflection ; and the synchronization of time bases. The final chapter, which might more appropriately have been included in an appendix, deals with the use of a time base for frequency division, A valuable feature of the book is the inclusion of seven appendices dealing with apparatus and circuits associated with time bases. The latter include differentiating, integrating and phase-shifting circuits and methods for generating square waves.

In a number of cases where typical values of components for various circuits are given, it would have been of assistance to the general reader if rather more information on the performance (e.g. frequency) of the circuit having these components had been given.

J. S. MCPETRIE.

Introduction to Atomic Physics, by S. TOLANSKY. Pp. xii+343. (London: Longmans, Green and Co., Ltd., 1942.) 15*s*. net.

Some of the many errors in this book are given in an erratum list of twenty-one items. The application, however, of such a corrigendum as " delete *from their ranges* " from " the kinetic energies of the neutron and hydrogen [i.e. proton] can be measured from their ranges " renders the remaining text rather meaningless to the student, who is left with no indication as to how the energy of the neutron was estimated in the Chadwick-Goldhaber experiment. Unfortunately, with so many mistakes this book is not one which can be recommended to students. Nothing short of careful re-writing can make it suitable for teaching purposes. It is to be hoped that the author will be able to see his way to such a revision, for there is, as he states, a real need for such a book as the one to which he has already laid a not inconsiderable foundation. W. B. M.

Copernicus, by Sir HAROLD SPENCER JONES, F.R.S., Astronomer Royal. Pp. 30. (Cardiff: Press Board of the University of Wales, University College of South Wales and Monmouthshire, 1943.) 1*s*. 6*d*. net.

After the recent spate of Copernicus quadricentennial commemorative volumes, this unpretentious pamphlet, which, unlike some of the others, savours nothing of political propaganda, comes with welcome freshness. The Astronomer Royal's Selby Lecture for 1943 is a straightforward account of the life of the great astronomer and of the profound revolution which his *De Revolutionibus* wrought in man's conception of the universe. It contains, incidentally, an admirable summary of mediaeval cosmology which serves as an introduction to a full discussion of the heliocentric theory ; and an equally readable account of the vicissitudes which that theory met in the century after its first appearance. Students of astronomy and those interested in the history of scientific thought will alike profit from a perusal of the booklet.

The lecture is quite unmistakably the work of an astronomer and not of a philosopher nor of a historian. It is none the worse for that. A. H.

Nuclear Physics (*University of Pennsylvania Bicentennial Conference*), by E. FERMI, G. BREIT, I. I. RABI, E. P. WIGNER, J. R. OPPENHEIMER and J. H. VAN VLECK. Pp. 68. (Pittsburg: University of Pennsylvania Press, 1941; London: Sir Humphrey Milford at the Oxford University Press.) 4*s*. 6*d*.

Of the six lectures delivered at the Bicentennial Conference on Nuclear Physics in the University of Pennsylvania, three are more descriptive in style, and appeal to the non-specialist, the remaining three being rather more technical in nature.

To the first group belongs Fermi's paper on neutron-induced radioactivity in heavy elements, which is condensed to four pages and yet is very readable. An account by Rabi of the magnetic dipole and electric quadripole moments of the light nuclei shows how these data confirm the general trend of current theories, but leave many features to be accounted for by future improved theories. The most notable contribution in this group is Oppenheimer's paper on the meson theory of nuclear forces. This goes to the central problem of modern nuclear physics, as yet essentially unsolved, and must therefore deal mostly with incomplete theories or with failures. I do not know whether to admire the paper more for capturing the reader's interest without promising a solution of the problem, or for writing of such abstract matters in the brief available space in such admirable language.

Breit shows, in a more technical account, how it was possible to derive the magnitude and range of the nuclear force between protons from measurements on the scattering of protons by hydrogen. Wigner gives an approximate theory of the binding energies of light nuclei, using, besides symmetry relations, some very simplified assumptions on the nature of the intranuclear forces. The last paper, by van Vleck, deals with the very attractive border-line between nuclear and atomic physics, viz. the magnetic scattering of neutrons, and its dependence on the atomic arrangement.

The whole collection gives a lively picture of some aspects of the front line of nuclear research, and the reader, who must feel regret at not having attended the Conference, is set speculating whether the name " Bicentennial Conference " means what it says and what will be the state of the subject when the Conference next meets in 2141. R. P.

Reports on Progress in Physics: Volume IX (1942–43). General Editor, W. B. Mann. Pp. iv+353. (London: The Physical Society, 1943.) 25*s*.

On account of the need for economy of time and material the annual publication of the *Reports* has been temporarily abandoned, and the present volume accordingly covers the two years 1942 and 1943.

The volume opens with a series of ten reports (100 pages) by members of the Gassiot Committee of the Royal Society: *Atmospheric physics and chemistry* (4 pages), by S. Chapman; *The spectrum of the Sun in the far ultra-violet* (5 pages), by A. Hunter; *Absorption spectra and absorption coefficients of atmospheric gases* (8 pages), by W. C. Price; *The infra-red spectra of atmospheric gases other than water vapour* (11 pages), by G. B. B. M. Sutherland and G. S. Callendar; *The absorption of water vapour in the far infra-red* (13 pages), by T. G. Cowling; *The spectrum of the night sky* (10 pages), by R. W. B. Pearse; *Ionization phenomena in the earth's atmosphere* (10 pages), by J. Sayers; *The properties of neutral and ionized atomic oxygen and their influence on the upper atmosphere* (13 pages), by H. S. W. Massey and D. R. Bates; *Photochemical processes in an oxygen-nitrogen atmosphere* (17 pages), by C. H. Bamford; *The photochemistry of atmospheric oxygen* (9 pages), by S. Chapman.

The other reports in the new volume are: *Solar physics* (12 pages), by A. Hunter; *The structure and elasticity of rubber* (24 pages), by L. R. G. Treloar; *Recent research on counter tubes* (21 pages), by J. D. Craggs; *Insoluble monolayers at the air-water and oil-water interfaces* (19 pages), by A. E. Alexander; *Field emission of electrons* (21 pages), by R. O. Jenkins; *Physics and the search for oil* (30 pages), by J. McG. Bruckshaw; *Sound* (28 pages), by E. G. Richardson; *Experimental study of X-ray scattering in relation to crystal dynamics* (38 pages), by K. Lonsdale; *Theoretical investigations on the relation between crystal dynamics and X-ray scattering* (40 pages), by M. Born; *The physics of paintings* (15 pages), by F. I. G. Rawlins. An author index (5 pages) is provided.

With Volume IX is issued a *Corrigenda* slip for the *Report* by R. T. Birge on *The General Physical Constants* which appeared in Volume VIII (1941).

CORRIGENDUM

R. S. Vincent and G. H. Simmonds. *Proceedings*, this volume, part 5 (1 September 1943): Pp. 377, 379, 381, running title. *For* " Surface tension " *read* "tension in liquids".

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SIR CHARLES G. DARWIN, K.B.E., M.A., Sc.D., F.R.S.,
President of the Physical Society, 1941–3.

6 *November* 1942

Science Meeting. At the Science Museum, London S.W. 7, the President, Sir Charles Darwin, being in the Chair.

The following were elected to Fellowship : The Hon. John Benson, William Bowen, Desmond Alan Bullock, Raymond Niblett Crooks, Robert William Ditchburn, Richard John Munson, Ronald John Trott, Ta-Hang Wang, Eric John Baxter Willey.

It was announced that the Council had elected the following to Student Membership : Ray M. Boorman, John Reginald Brough, Frank Eric Claytor, William George Clouter, A. H. Coppard, Patricia Elizabeth Ferguson, J. A. F. Gerrard, Miriam Gilbert, Glynn Bennett Jeffery, Joan Kelly, R. C. Lane, Ronalde Philip Le Blanc, Emlyn Howard Lloyd, Thomas R. Lomer, Geoffrey George Parfitt, George S. Parry, George Pearce, H. D. G. Piggott, Anthony Fred Doran Pott, John Sadler Roberts, Harry Verdon Stopes-Roe, Paul Thistleton, Alan Alfred Ware, Harold Edgar Wilkinson.

The first Rutherford Memorial Lecture was delivered by Professor H. R. Robinson, D.Sc., Ph.D., F.R.S., who took as his subject " Rutherford : life and work to the year 1919, with personal reminiscences of the Manchester period ".

20 *November* 1942

Science Meeting. At the Imperial College, London S.W. 7, Mr. T. Smith, Past President, being in the Chair.

John Home Dickson, Frank Taylor Farmer and Kenneth Edmond Brian Jay were elected to Fellowship.

It was announced that the following had been elected to Student Membership : Peter Milton Barham, Basil Jeffrey Benzimra, Bruce Alexander Bilby, Donald Eustace Blackwell, J. B. S. Brabham, C. J. B. Bradish, Basil Hugh Briggs, W. H. Butters, Peter Crombie Calder, Douglas Chaplin, Kenneth Henry Clarke, Robert F. Crocombe, Robert Alan Croft, Edward Thomas Emms, John Gorham, Douglas Hardy, Roy G. B. Hine, Alfred Peter Henry Jennings, Eleanor M. Jones, James Tyldesley Kendall, Raymond Douglas Lowde, Donald Douglas McVicar, Clifford Charles Joseph Milner, Roy Morris, Donald Vernon Osborne, Charles Arthur Padgham, Dennis Pallant, Peter Llewellyn Pennington, Bernard John Perrett, Percy Charles Ruggles, W. E. Shackcloth, Ernst Helmut Sondheimer, T. A. J. Stocker, Dennis John Tarran, Harold A. Waters, Donald Whittaker, Geoffrey P. L. Williams and Leonard Wiseman.

A discussion was held on " Scientific films and their use in the teaching of physics ", the opening paper, by W. Ashhurst, M.Sc., being illustrated by films of various types.

27 *November* 1942

The fifth meeting of THE OPTICAL GROUP. At the Imperial College, London S.W. 7, Dr. A. O. Rankine being in the Chair.

The following papers and exhibits dealing with *Optical materials other than glass* were read and discussed :

" Plastic spectacle lenses ", by H. H. Emsley, B.Sc.

" Recent optical materials and their possible applications ", by B. K. Johnson.

" Plastics and the optical industry ", by W. G. Wearmouth, Ph.D.

A lecture on " The effects of grains in photographic materials " was delivered by E. W. H. Selwyn, B.Sc., and followed by an informal discussion.

4 *December* 1942

Science Meeting. At the Science Museum, London S.W. 7, the President, Sir Charles Darwin, being in the Chair.

The following were elected to Fellowship : Edward Gordon Butcher, John Anthony Chaldecott (transferred from Student Membership) and Donald George Marshall.

To mark the tercentenary of Newton's birth, a lecture on " Newton " was delivered by Professor E. N. da C. Andrade, Ph.D., D.Sc., F.R.S.

9 *December* 1942

Science Meeting. At the Cavendish Laboratory, Cambridge, Sir Arthur Eddington, Past President, being in the Chair.

Professor Andrade repeated the Newton Tercentenary Lecture.

11 *December* 1942

The tenth meeting of THE COLOUR GROUP. At the Lighting Service Bureau, Savoy Hill, London W.C. 2, Dr. W. D. Wright being in the Chair.

The following papers on *Fluorescent materials* were read and discussed:

" The chemical and physical properties of luminescent materials ", by J. W. Strange, Ph.D.

" The Kodak fluorescence process of automatic colour correction ", by F. W. Coppin.

" Commercially useful fluorescent substances ", by H. G. Jenkins, M.Sc.

" The photometry and colorimetry of fluorescent lamps ", by G. T. Winch.

22 *January* 1943

The sixth meeting of THE OPTICAL GROUP. At the Imperial College, London S.W. 7, Dr. A. O. Rankine being in the Chair.

The following papers on *The resolution of optical images* were read and discussed :

" The Airy disc formula for systems of high relative aperture ", by H. H. Hopkins, B.Sc.

" Resolution in dark-ground illumination ", by K. B. E. Merling.

" Resolution in photo-micrography ", by J. Smiles.

" The theory of the microscope. III, Boundary waves in dark-ground illumination ", by L. C. Martin, D.Sc.

19 *February* 1943

Science Meeting. At the Imperial College (City and Guilds College), London S.W. 7, the President, Sir Charles Darwin, being in the Chair.

Richard Christian Hawkes was elected to Fellowship.

Professor C. L. Fortescue gave an account of the war-time work of the Electrical Engineering Department of the College, with special reference to the course of instruction in Radio. Members of the staff and students of the Department exhibited the work in progress and gave demonstrations.

3 *March* 1943

The eleventh meeting of THE COLOUR GROUP. At the Science Museum, London S.W. 7, Dr. W. D. Wright being in the Chair.

A demonstration of " A Cellon portable colour matcher " was given by E. R. Wells, B.Sc.

A paper on " The significance and limitations of the C.I.E. standard observer tables ", by J. Guild, was read and discussed.

The meeting was preceded by the third *Annual General Meeting of the Colour Group* for the presentation of the Honorary Secretary's statement for 1942–43 and the election of Officers and Committee for 1943–44.

9 *April* 1943

At the Imperial College, London S.W. 7, the President, Sir Charles Darwin, being in the Chair.

The following were elected to Fellowship, the first sixteen being transferred from Student Membership : Richard Frank Barrow, Clement John Danby, Hugh Alexander Dell, Douglas Hugh Everett, Harold Hutchinson, James Tyldesley Kendall, Graham

Frederick Komlosy, Hugh Russell Letham Lamont, John Watson Leech, Stanley Naylor, Bernard Edward Noltingk, Arthur Charles Reid, Bernard Walter Soole, Arthur Geoffrey Stainsby, John Lawrence Tearle, Stanley Gordon Tomlin, James Arthur Ankerson, John Edward Bowman, Herbert John Dunster, Werner Ehrenberg, Thomas Sidney England, Malcolm Charles Marsh, Violet Olive Shirtliffe, Dennis Edward Skelton, John Leslie Thompson.

It was announced that the following had been elected to Student Membership : Raymond K. Appleyard, B. H. Chirgwin, George Cowper, Duncan S. Davies, Brian Philip Day, Eric Foster, Lionel Gilbert, Leonard A. Green, Oliver Samuel Heavens, Christopher Guy Ardern Hill, J. J. Kipling, Henry Lawton, James William McHugo, Arthur H. Rowden, J. G. Schwenger, K. W. Sykes, Eva Edith Vago, Bernard Miles Wheatley, Garth Angus Wheatley, John Reginald Whitefield, Richard Wallace Whorlow.

The nineteenth Duddell Medal was presented to Dr. C. R. Burch in recognition of his work on high-vacuum equipment. The medallist delivered an Address entitled " A technologist looks at the future ".

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16 *April* 1943

The seventh meeting of THE OPTICAL GROUP. At the Imperial College, London S.W. 7, Dr. A. O. Rankine being in the Chair.

A paper on " The limit of visual resolution ", by E. W. H. Selwyn, B.Sc., was read and discussed.

A discussion was held on " The designing of systems with non-spherical surfaces ", the opening paper being by Instr.-Capt. T. Y. Baker, B.A., and further contributions by J. H. Jeffree, E. H. Linfoot, L. C. Martin and T. Smith.

The meeting was preceded by the second *Annual General Meeting of the Optical Group* for the presentation of a report on the work of the first year and the election of Officers and Committee for 1943–44.

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18 *May* 1943

Annual General Meeting. At the Royal Institution, Albemarle Street, London W. 1, the retiring President, Sir Charles Darwin, being in the Chair.

The minutes of the previous Annual General Meeting were read and accepted as correct.

The reports of the Council and the Honorary Treasurer and the Annual Accounts for 1942 were adopted.

The Officers and Council and the Auditors for 1943–44 were elected.

Votes of thanks were accorded to the Rector and Governing Body of the Imperial College, the Managers of the Royal Institution, the Director of the Science Museum, and the Electric Lamp Manufacturers' Association for excellent accommodation at meetings, to the retiring Officers and Council, and to Professor A. F. C. Pollard for preparing the U.D.C. Index Slips for the *Proceedings*.

The meeting was preceded by an *Extraordinary General Meeting* for the election of Professor Percy Williams Bridgman as an Honorary Fellow of the Society.

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18 *May* 1943

Science Meeting. At the Royal Institution, Albemarle Street, London W. 1, the new President, Professor E. N. da C. Andrade, being in the Chair.

The following were elected to Fellowship, the first five being transferred from Student Membership : William Denis Hammond Blackman, Frank Eric Claytor, Wilfred Percy Fletcher, Douglas Edward Lewis, Brian Percival Mullins, William Ashhurst, Frank Kennedy-Newman, Arthur Henry Lee.

The twenty-seventh Guthrie Lecture was delivered by Professor E. T. Whittaker, M.A., Sc.D., LL.D., F.R.S., who took as his subject " Chance, freewill and necessity in the scientific conception of the Universe ".

4 *June* 1943

Science Meeting. At the Science Museum, London S.W. 7, the President, Prof. E. N. da C. Andrade, being in the Chair.

The following were elected to Fellowship : Nicholas Kemmer, Erich Ernst Schneider, Charles Frederick Sharman, Robert Street, Gordon B. B. M. Sutherland, Trevor Hugh Turney.

It was announced that the following had been elected to Student Membership : Eileen Nora Bullerwell, Alfred Ernest Kay, Kenneth Elmslie Munn and Patrick Lever Willmore.

The thirteenth Thomas Young Oration was delivered by Professor F. C. Bartlett, C.B.E , M.A., F.R.S., who took as his subject " Some current problems in visual functions and visual perception ".

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18 *June* 1943

The eighth meeting of THE OPTICAL GROUP. At the Imperial College, London S.W. 7, Professor L. C. Martin being in the Chair.

A lecture on " Optical aids to teaching " was delivered by H. E. Dance.

There followed an exhibition and demonstration of new optical projection apparatus suitable for educational purposes.

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16 *July* 1943

Science Meeting. At the Royal Institution, Albemarle Street, London W. 1, the President, Professor E. N. da C. Andrade, being in the Chair.

The following were elected to Fellowship : Victor Ivor Little, William Clifford Morgan (transferred from Student Membership), Rudolf Peierls, Guthrie Rae Petrie, George Dawson Preston, John Alan Richardson.

The second Charles Chree Medal and Prize were presented to Professor (now Brigadier) Basil F. J. Schonland, O.B.E., M.A., Ph.D., F.R.S., in recognition of his work on thunderstorms and the lightning stroke.

The Charles Chree Address on " Thunderstorms and their electrical effects " was delivered by Brigadier Schonland.

-REPORT OF COUNCIL FOR THE YEAR ENDED 31 DECEMBER 1942

Presented at the Annual General Meeting, 18 *May* 1943

INTRODUCTORY AND GENERAL

The Council again has to report on a period of remarkable growth, activity and prosperity of the Society. The membership shows a greater increase in 1942 than in any previous year, with the single exception of 1932, when the Optical Society and the Physical Society of London combined to form the present Physical Society ; science meetings were held frequently and attended well ; the Colour and Optical Groups have been conspicuously successful and cordially supported ; the sales of the *Proceedings*, the *Reports on Progress in Physics* and the earlier special publications of the Society have been highly satisfactory ; and the financial position of the Society has further improved.

In December 1942 the Council, with the Councils of the Faraday Society, the Royal Meteorological Society and the British Institute of Radiology, gave to the Institute of Physics twelve months' notice of the termination of the existing scheme of reduced conjoint subscriptions ; this is to be replaced as from 1 January 1944 by a simplified scheme, details of which are still under discussion.

Jointly with the Science Masters' Association the Society has set up a Standing Committee for Co-ordination and Guidance in the Teaching of Physics in Schools, the first meeting of which was held on 12 August 1942. The Committee is to be so constituted that it may serve in a consultative capacity to all bodies and individuals concerned with the teaching of elementary physics. Typical of its functions may be : the making of recommendations on definitions, statements of laws, notation and conventions ; the discussion of general and special problems of laboratory equipment during and after the war ; and the discussion of syllabuses with examining bodies and with authors and publishers of text-books.

The President, Sir Charles Darwin, returned from America early in the year.

The Society's representatives on other Bodies were the same as in 1941.

In fulfilment of a condition of the Society's occupation of the upper two floors of 1 Lowther Gardens, Exhibition Road, London, S.W.7, fire-watching was undertaken throughout the year; the Council again records its thanks to Dr. and Mrs. W. Jevons for accepting responsibility for this, and to Dr. and Mrs. R. W. B. Stephens for relieving them in this duty.

MEETINGS

An Annual General Meeting was held at the Imperial College on 2 October 1942 for the presentation of the 1941 Reports of the Council and the Honorary Treasurer and for the election of the Officers and Council for 1942–43.

Thirteen Science Meetings were held during the year : six at the Imperial College, three at the Science Museum, two at the Cavendish Laboratory, Cambridge, one at Rugby School and one at the Lighting Service Bureau of the Electric Lamp Manufacturers' Association. The war-time series of " lecture-surveys " and discussions was continued by Dr. Kathleen Lonsdale on " X-ray Study of Crystal Dynamics " (in London and again, with modifications, at Cambridge), by Professor W. V. Mayneord on " Measurement of Radiation for Medical Purposes ", and by Dr. E. G. Richardson on " Turbidity Measurements by Optical Means " ; these three lecture-surveys have been published in the *Proceedings* (**54**, 314 and 405 (1942), and **55**, 48 (1943)). Three of the meetings were held jointly with other bodies, namely, with the Cambridge Philosophical Society for the repetition of Mrs. Lonsdale's lecture, with the Royal Meteorological Society for a discussion on " Emission and Absorption of Radiation in the Atmosphere ", and with the Science Masters' Association for a discussion on " Some Aspects of the Teaching of Physics in Schools ". One meeting was devoted to a demonstration and discussion of " Scientific Films and their Use in the Teaching of Physics ", and one (of the kind that was usual before the war) to the reading of papers and a demonstration. The attendances were satisfactory at most of the ordinary meetings and very large at the meetings for the special lectures reported in the following paragraphs.

GUTHRIE LECTURE

The twenty-sixth Guthrie Lecture was delivered on 20 March 1942 at the Science Museum by Sir Edward V. Appleton, who took as his subject " Ionospheric Influences on Geomagnetism ".

RUTHERFORD MEMORIAL LECTURE

The first Rutherford Memorial Lecture was delivered by Professor H. R. Robinson at the Science Museum on 6 November 1942, the title of the lecture being " Rutherford : Life and Work to the Year 1919, with Personal Recollections of the Manchester Period " (*Proceedings*, **55**, 161 (1943)).

NEWTON TERCENTENARY LECTURE

In celebration of the tercentenary of Newton's birth a lecture entitled " Newton " was delivered by Professor E. N. da C. Andrade at the Science Museum on 4 December 1942 and at the Cavendish Laboratory on 9 December 1942 (*Proceedings*, **55**, 129 (1943)).

DUDDELL MEDAL

The speeches delivered by Viscount Halifax and Dr. Coolidge at the presentation of the eighteenth Duddell Medal at Baltimore on 1 May 1942 have been published in the *Proceedings* (**54**, 527 (1942)).

The Council awarded the nineteenth Duddell Medal to Dr. C. R. Burch in recognition of his work on high-vacuum equipment. The presentation of the Medal, which could not be made at a Science Meeting in 1942, was deferred until 9 April 1943.

PROCEEDINGS

Volume **54** (1942) of the *Proceedings* has been published in the usual six two-monthly parts, but as was foreseen at the time of preparation of Council's Report for 1941, it is considerably smaller than either of the earlier war-time volumes, on account of the present limitation in the quantities of printing paper licensed by the Paper Control of the Ministry of Supply. In compliance with Paper Control regulations less space was devoted to advertisements in Volume **54** than in Volume **53** (1941).

Losses at sea, occasionally affecting relatively large consignments, and increased circulation both to the growing membership and to subscribers outside the Society, have together resulted in a shortage of stock of the first three Parts of the 1942 volume of the *Proceedings*. The Council therefore appealed to members of the Society to return copies no longer required by them, and is grateful to those members who generously responded to the appeal. The Officers and Council have decided that, when the supply of paper allows, those Parts of which stocks have become exhausted or very low shall be reprinted and a larger number of future issues shall be printed.

REPORTS ON PROGRESS IN PHYSICS

The sales of Volume **8** (1941) of the *Reports*, which was published in April 1942, have been highly satisfactory, as also have those of all the earlier volumes that are still available, especially Volume **7** (1940). There is still a considerable demand for copies of Volumes **1** (1934), **2** (1935) and **4** (1937), which have been out of print for several years. The sales of the *Reports* in the United States of America have continued to increase, and the Council again records its thanks to Miss Mitchell, Publications Manager of the American Institute of Physics, for her very kind and effective help in making our *Reports* easily available to American physicists by direct supply from the New York office of the Institute.

The demand for the whole series is expected to increase at the end of the war, and it is to be regretted that the supply of paper has not permitted the printing of larger editions or the reprinting of the earlier volumes. In view of the present restrictions it has been decided that all the paper allowed for the *Reports* in 1942 and 1943 shall be used for a single volume (Volume **9**, already in active preparation) covering the two years instead of 1942 only. This course also effects an economy in the Society's stock of green binding cloth, which is now very difficult to obtain.

MEMBERSHIP

As the following table shows, the Fellowship increased in 1942 by nearly 5 per cent, and the Student Membership by about 37 per cent, making an increase of over 9 per cent in the total membership. The figure of 1247 at the end of December was by far the highest reached by the Society, and has been followed by a further increase up to the time of preparation of this Report. The numbers of newly elected Fellows, of Student Members transferred to Fellowship, and of newly elected Student Members were all larger than in any one year in the history of the Society.

Membership Roll (corrected figures)		Hon. Fellows	Hon. Fellows, Optical Society	Ex-officio Fellows	Fellows	Student Members	Total
Totals, 31 Dec. 1941		11	3	4	953	169	1140
Changes during 1942	Elected				57 } 77	96	
	Transferred				20 }	20 }	
	Deceased	1			17 }		
	Resigned or lapsed				14 } 32	4 } 33	
	Suspended				1 }	9 }	
	Net increase	−1			45	63	107
Totals, 31 Dec. 1942		10	3	4	998	232	1247

OBITUARY

The Council records with regret the deaths of the following Fellows:—Mr. S. G. Barker, Mr. W. A. Benton, Sir William H. Bragg (Past President), Mr. L. K. Brata, Mr. C. R. Darling (Past Vice-President), Mr. N. Eumorfopoulos, Professor C. C. Farr, Mr. T. Harris, Sir Joseph Larmor, Sir Henry A. Miers, Mr. B. M. Neville, Professor Jean Perrin (Honorary Fellow), Mr. S. Rennie, Mr. J. Rheinberg, Professor A. L. Selby, Mr. J. H. Sutcliffe (Past President of the Optical Society) and Mr. F. P. Whitehead.

COLOUR GROUP

At the second Annual General Meeting of the Colour Group, which was held at the Imperial College on 11 February 1942, Dr. W. D. Wright was re-elected as Chairman, Mr. H. D. Murray as Honorary Secretary, and the Committee for 1942–43 was elected.

Five Science Meetings of the Group were held in 1942 : on 11 February at the Imperial College ; on 27 March at the Midland Hotel, Manchester ; on 27 May at the Royal Photographic Society ; on 11 September at the Imperial College ; and on 11 December at the Lighting Service Bureau of the Electric Lamp Manufacturers' Association. The average attendance was about 64. The titles of the lectures delivered and of the papers read and discussed at these meetings are set out, with the names of the authors, in the pages immediately preceding Council's Reports in the November 1942 and November 1943 issues of the *Proceedings*.

The Chairman's Address on "Research on Colour Physics at South Kensington" and four of the 1942 lectures and papers have already been, or shortly will be, published in the *Proceedings* (**54,** 303 (1942) ; **55,** 1, 81, 364 and 371 (1943)) ; one paper, with the ensuing discussion, has been published in the *Journal of the Oil and Colour Chemists' Association* (**25,** 227 (1942)) ; and brief accounts of a lecture on the Teaching of Colour in Schools and a discussion on Fluorescent Materials have appeared in *Nature* (**150,** 422 (1942) ; **151,** 73 (1943)).

The two sub-committees dealing with Colour Blindness in Industry and Colour Terminology have continued their work.

The membership of the Group on 31 December 1942 was as follows :

Members of the Physical Society	69
Members of other participating bodies	61
Members of six firms subscribing for sustaining membership	15
Other individual members	4
Total ..	149

OPTICAL GROUP

The Inaugural General Meeting of the Optical Group was held in the Science Museum on 6 March 1942, when a draft Constitution was discussed and adopted, and the Chairman (Dr. A. O. Rankine), Honorary Secretary (Prof. L. C. Martin) and Committee for 1942–43 were elected.

Five Science meetings of the Group were held in 1942, all in the Physics Department of the Imperial College : on 6 March, 15 May, 17 July, 18 September and 27 November. Each meeting consisted of an afternoon and an early evening session in order to provide sufficient time for discussion of the lectures, papers and demonstrations. The attendances were very satisfactory, the average being about 50. The titles and authors are given in the pages immediately preceding the Reports of Council in the November 1942 and November 1943 issues of the *Proceedings.*

The inaugural lecture delivered by Dr. W. M. Hampton and eight of the other 1942 lectures and papers have already been, or shortly will be, published in the Society's *Proceedings* (**54,** 391 (1942) ; **55,** 15, 102, 182, 257, 291, 301, 314 and 433 (1943)), and one demonstration has been published in *The Proceedings of the British Society for International Bibliography* (**4,** 25 (1942)). Arrangements have been made for the supply of off-prints of the published papers and discussions to members of the Group.

The membership of the Group on 31 December 1942 was as follows :

Members of the Physical Society	112
Members of other participating bodies	95
Members of twenty-two firms subscribing for sustaining membership	85
Other individual members	11
Total ..	303

REPORT OF THE HONORARY TREASURER FOR THE YEAR ENDED 31 DECEMBER 1942

Presented at the Annual General Meeting, 18 *May* 1943

The excess of income over expenditure was about £649 in December 1942, as compared with £140 a year earlier, the total expenditure being roughly £30 less than in 1941, and the receipts £539 greater. The saving is partly due to the fact that the regulations governing the supply and use of paper did not permit the printing of sufficient copies of Vol. 54 of the *Proceedings* to keep pace with the greatly increased demand, a fact to which reference is made in the Council's Report.

The most satisfactory index of the improvement in the affairs of the Society is to be seen in the increases of income from entrance fees, from members' annual subscriptions, and from sales of the *Reports on Progress in Physics.*

A further publications grant of £200 by the Royal Society from the Rockefeller Foundation Gift is gratefully acknowledged. In due course this will be expended on the reprinting of parts of the 1942 *Proceedings* which are out of print.

The stock of publications, the total value of which has not changed very much, has not been re-assessed. No alteration of the Society's investments was made in 1942, the total market value of which increased by about £180 during the year.

(Signed) CLIFFORD C. PATERSON,
Honorary Treasurer.

9 *April* 1943.

INCOME AND EXPENDITURE ACCOUNT FOR THE YEAR ENDED 31 DECEMBER 1942

1941 £	EXPENDITURE	£	s.	d.	£	s.	d.
	To						
201	"Science Abstracts"				320	9	10
	Normal Publications:						
1905	"Proceedings"	1512	13	10			
38	Agenda Papers and Notices	46	17	6			
693	"Reports on Progress in Physics"	833	19	10			
					2393	11	2
234	Postage on Publications and General Correspondence				253	12	2
44	Expenses at Meetings				63	17	0
40	Honoraria to Special Lecturers				72	10	0
	Administration Expenses:						
761	Secretarial and Clerical Assistance and Office Expenses	850	10	2			
95	1 Lowther Gardens (including Air Raid Precautions)	87	1	5			
					937	11	7
140	*Balance, being excess of Income over Expenditure, carried forward to Accumulated Fund*				648	12	5
£4151					£4690	4	2

1941 £	INCOME	£	s.	d.	£	s.	d.
	By						
	Subscriptions:						
36	Entrance Fees	56	14	0			
1692	Fellows	1756	19	11			
85	Student Members	123	0	6			
23	Colour Group	24	4	0			
—	Optical Group	56	16	0			
112	For "Science Abstracts" and Advance Proofs	114	11	1			
					2132	5	6
	Sales:						
797	"Proceedings"	764	11	8			
616	"Reports on Progress in Physics"	862	13	1			
48	Special Publications	104	13	4			
					1731	18	1
236	Advertisements in "Proceedings"				332	2	2
286	Dividends from Investments				283	18	5
20	Transfer from Life Composition Fund of amounts paid by Fellows now deceased				10	0	0
200	Publications Grant by the Royal Society from the Rockefeller Foundation Gift				200	0	0
£4151					£4690	4	2

BALANCE SHEET AS ON 31 DECEMBER, 1942

LIABILITIES

	£ s. d.	£ s. d.
Sundry Creditors		181 12 4
Life Compositions :		
As on 31 December 1941	1481 0 0	
Add Payments during year	63 0 0	
	1544 0 0	
Less Transfer to Income and Expenditure Account	10 0 0	
		1534 0 0
Subscriptions received in advance :		
Members	11 11 0	
Publications	73 2 10	
		84 13 10
SPECIAL FUNDS :		
W. F. Stanley Trust Fund	259 0 0	
Duddell Memorial Trust Fund	369 1 3	
"Progress Reports" Reserve Account	83 1 0	
Herbert Spencer Legacy	239 7 6	
Charles Chree Medal and Prize Fund	1949 9 2	
		2899 18 11
GENERAL FUND :		
As on 31 December 1941	4346 19 10	
Add Balance of Income and Expenditure Account	648 12 5	
		4995 12 3
		£9695 17 4

ASSETS

	£ s. d.	£ s. d.
Investments at Market Value on 31 December 1939 :		
W. F. Stanley Trust Fund	259 0 0	
Duddell Memorial Fund	374 0 0	
Charles Chree Medal and Prize Fund	1866 10 0	
General Fund	5917 10 0	
		8417 0 0
(Market Value on 31 December 1942 : £9830.)		
Dividends due from Investments		54 12 1
Inland Revenue—Income Tax recoverable for 1942		55 10 10
Subscriptions due		67 8 3
Sundry Debtors		205 5 8
Stock of Paper and Binding Material		166 19 9
Cash at Bank	413 17 10	
,, *in Post Office Savings Bank Account*	315 2 11	
		729 0 9
		£9695 17 4

We have audited the above Balance Sheet and have obtained all the information and explanations we have required. We have verified the bank balances and the Investments. In our opinion such Balance Sheet is properly drawn up so as to exhibit a true and correct view of the state of the Society's affairs according to the best of our information and the explanations given to us and as shown by the books of the Society.

SPENCER HOUSE, SOUTH PLACE, E.C. 2.
6th April 1943.

KNOX, CROPPER & Co.,
Chartered Accountants.

LIFE COMPOSITION FUND ON 31 DECEMBER 1942

	£	s.	d.
28 Fellows paid £10	280	0	0
1 Fellow paid £15	15	0	0
14 Fellows paid £21	294	0	0
30 Fellows paid £31 10s.	945	0	0
	£1534	0	0

SPECIAL FUNDS

W. F. STANLEY TRUST FUND

	£	s.	d.		£	s.	d.
Carried to Balance Sheet	259	0	0	£300 Southern Railway Preferred Ordinary Stock	199	0	0
				£442 Southern Railway Deferred Ordinary Stock	60	0	0
	£259	0	0		£259	0	0

DUDDELL MEMORIAL TRUST FUND

CAPITAL

	£	s.	d.		£	s.	d.
Carried to Balance Sheet	374	0	0	£400 3½% War Loan Inscribed "B" Account	374	0	0

REVENUE

	£	s.	d.		£	s.	d.
Honorarium	20	0	0	Balance on 31 December 1941	5	4	9
Medal and Certificate	4	3	6	Interest on War Loan	14	0	0
				Balance carried to Balance Sheet	4	18	9
	£24	3	6		£24	3	6

"PROGRESS REPORTS" RESERVE ACCOUNT

	£	s.	d.		£	s.	d.
Balance carried to Balance Sheet	83	1	0	Balance on 31 December 1941	83	1	0

HERBERT SPENCER LEGACY

	£	s.	d.		£	s.	d.
Balance carried to Balance Sheet	239	7	6	Balance on 31 December 1941	239	7	6

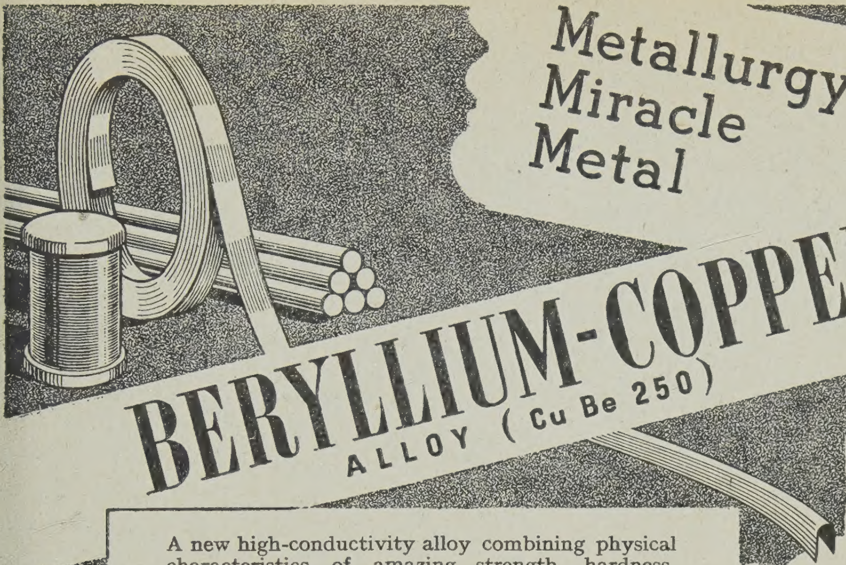
CHARLES CHREE MEDAL AND PRIZE FUND

CAPITAL

	£	s.	d.		£	s.	d.
Balance carried to Balance Sheet	1865	16	4	Balance on 31 December 1941	1865	16	4

REVENUE

	£	s.	d.		£	s.	d.
Balance carried to Balance Sheet	83	12	10	Balance on 31 December 1941	14	15	8
				Interest on Investments	68	17	2
	£83	12	10		£83	12	10



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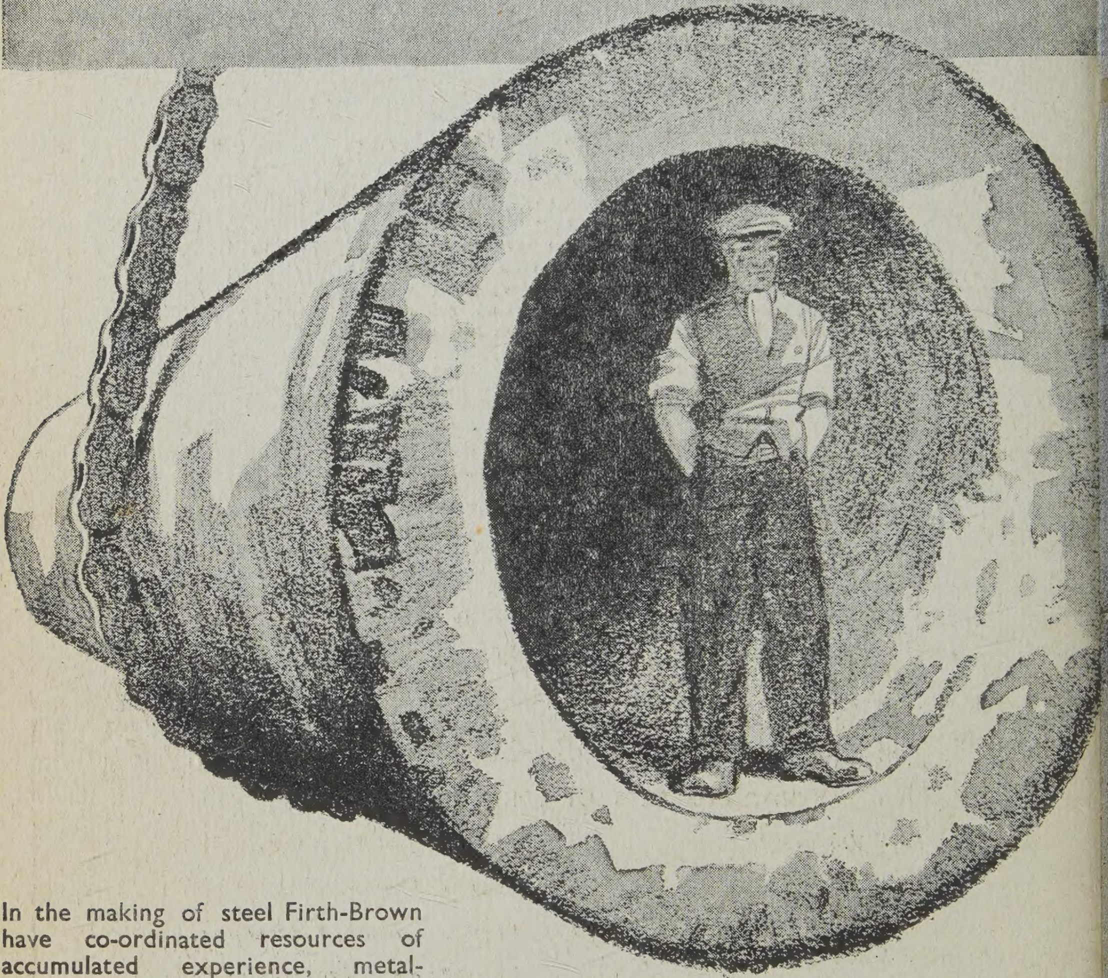

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